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HALF-YEARLY ABSTRACT  
OF THE  
MEDICAL SCIENCES.

JULY—DECEMBER,

1855.

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THE  
HALF-YEARLY ABSTRACT  
OF THE  
MEDICAL SCIENCES:

BEING  
A PRACTICAL AND ANALYTICAL DIGEST OF THE CONTENTS OF THE PRINCIPAL  
BRITISH AND CONTINENTAL MEDICAL WORKS PUBLISHED  
IN THE PRECEDING SIX MONTHS:

TOGETHER WITH A  
SERIES OF CRITICAL REPORTS ON THE PROGRESS OF MEDICINE AND  
THE COLLATERAL SCIENCES DURING THE SAME PERIOD.

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Apparatu nobis opus est, et rebus exquisitis undique et collectis, arcessitis, comportatis.  
CICERO.

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# HALF-YEARLY ABSTRACT

OF

THE MEDICAL SCIENCES,

*&c. &c.*

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## PART I.

PRACTICAL MEDICINE, PATHOLOGY, & THERAPEUTICS.

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### SECT. I.—GENERAL QUESTIONS IN MEDICINE.

#### (A) HYGIENE.

ART. 1.—*Results of Vaccination in the Prussian Army.*  
By Dr. SCHILLING.

(*Vierteljahrschr. für g. u. Off. Med.*, Oct. 1854; and *Phil. Med. Examiner*, July, 1855.)

THE results of re-vaccination in the Prussian army, from 1833 to 1852, both years inclusive, are contained in the following tables. There is no information, unfortunately, as to the time over which the protective powers of vaccination may be supposed to extend—for the greater number of soldiers in this army are renewed every three years; but one important fact is disclosed, and this is the progressive increase in the number of successful vaccinations—a fact which would seem to argue a growing susceptibility to the influence of the vaccine virus.



*Results of Re-vaccination in the Army.*

1	2	3			4			5		6				7			8
		a.	b.	c.	a.	b.	c.	a.	b.	a.	b.	c.	d.	a.	b.	c.	
Year.	Number of Vaccinated.	Decided	Un- certain.	None.	Regular	Ir- regular.	No result.	With success.	With- out success.	1-5.	6-10.	11-20.	21-30.	Vari- cella.	Vario- loid.	Genuine variola.	Out of 100, true pus- tules ap- peared in
1833	48,478	37,286	7,641	3,551	15,269	12,293	21,003	784	3,377	6,586	4,854	3,217	612	54	50	20	over 33
1834	44,454	33,634	7,134	3,686	16,679	12,287	15,483	856	3,654	6,763	5,028	4,088	230	46	31	2	" 39
1835	39,192	30,242	6,233	2,717	15,315	10,135	13,742	1,465	7,946	6,298	4,990	3,417	610	21	14	1	almost 43
1836	42,124	32,635	6,645	2,844	18,136	9,940	14,048	1,569	8,205	7,311	5,647	4,418	769	14	8	—	over 46
1837	47,258	37,299	6,903	3,056	21,308	10,557	15,393	2,243	9,771	9,174	6,414	4,767	953	14	7	—	" 40
1838	42,041	33,819	5,645	2,577	19,117	8,672	14,252	2,306	10,424	8,787	5,581	4,056	693	19	10	2	almost 51
1839	41,481	33,225	5,889	2,367	19,249	8,534	13,698	1,105	7,886	8,762	5,650	4,095	742	18	7	—	over 51
1840	43,522	34,573	6,177	2,772	20,952	8,829	13,750	2,831	8,958	10,021	5,875	4,171	825	7	2	1	" 54
1841	44,491	36,182	6,192	2,567	23,383	8,035	13,523	2,254	9,468	11,174	6,516	4,838	855	1	8	—	" 57
1842	42,582	33,185	6,751	2,646	21,865	8,056	12,661	2,029	9,536	10,697	6,223	4,200	745	9	4	—	" 58
1843	42,998	34,390	6,258	2,350	22,062	8,613	12,323	2,439	9,671	10,568	6,426	4,392	676	11	8	4	almost 57
1844	40,661	32,779	5,463	2,419	21,038	7,945	11,678	2,278	9,712	11,571	6,619	4,417	709	5	8	1	over 57
1845	42,671	33,813	6,041	2,817	22,214	8,764	11,693	2,749	9,974	12,208	6,944	4,917	894	2	3	—	" 58
1846	44,012	34,708	6,453	2,851	24,138	7,991	11,883	2,534	9,855	13,213	7,510	5,081	868	4	—	—	" 60
1847	43,596	34,264	6,405	2,927	25,544	7,425	10,627	2,718	8,952	13,295	8,164	5,767	1,036	—	—	—	almost 65
1848	28,859	22,386	4,211	2,262	16,882	4,404	7,573	1,579	5,786	9,161	5,134	3,513	653	6	1	—	64
1849	51,637	39,116	8,706	3,815	30,457	8,467	12,713	2,862	9,194	16,018	9,621	6,617	1,063	5	9	1	" 64
1850	44,539	33,466	7,053	4,040	25,030	7,509	12,000	2,355	8,766	12,461	7,964	6,092	868	10	22	—	61
1851	57,059	42,203	9,935	4,921	33,444	9,857	13,758	3,338	10,354	16,203	11,296	8,196	1,087	20	41	—	64
1852	27,775	21,195	4,242	2,338	17,782	3,920	6,073	1,468	4,639	8,413	5,967	4,304	564	5	7	—	69

"This summary shows the total number of vaccinations during twenty years to have been 253,380 (1st column). Among these, 429,864 yielded successful cases, regular in their course (column 4, *a*). Those remaining without result, but re-vaccinated afterwards, successfully, amounted to 43,770 (column 5, *a*). So that in all, 473,634 cases of genuine and regular vaccination were produced.

"Of those successively re-vaccinated during twenty years, 545 were attacked with (column 7)

Varicella . . . . .	271 . . . . .	Column 7, <i>a</i>
Varioloid . . . . .	241 . . . . .	" 7, <i>b</i>
Variola . . . . .	33 . . . . .	" 7, <i>c</i> .

"The year 1833 is strikingly remarkable as being by far the most unfavorable in its protective results. It would have been better to have omitted the first year, and only cited the last nineteen years, if I had only wished to produce the proofs of a statement, and not, at the same time, to make the utmost use of the whole of the materials at my command. But I must remember that this universal vaccination, without regard to whether it had been previously done or not, only commenced with the year 1834. A proper estimation of re-vaccination as it now exists in the army, can, in fact, therefore be obtained by taking into account the last nineteen years only.

"In these nineteen years, the number vaccinated upon their entrance into the army was 811,402 (column 1). The first vaccination was successful with 414,595 (column 4, *a*). Those unsuccessful at first, but successfully re-vaccinated, were 42,986 (column 5, *a*). The vaccination, therefore, took effect in a regular manner with 457,581. Of those successfully re-vaccinated during nineteen years, 421 were attacked in nineteen years

With Varicella . . . . .	217
Varioloid . . . . .	191
Variola . . . . .	13

"Of these 457,581 soldiers successfully vaccinated during nineteen years, 4 have died of variola.

"This result is indeed astonishingly favorable, for variolous diseases still exist and appear in all times and all places in our State, notwithstanding that compulsory vaccination and the extinction of prejudice have diminished from year to year the number of those who have avoided vaccination. An immunity from the contagion of variolous diseases exists neither in age, sex, race, nor constitution. The rate of mortality of the different epidemics, it is true, is very different, sometimes rising to 60 or 70 per cent., sometimes sinking to 15 per cent. of those attacked. The average mortality may be stated as about 30 per cent. The Prussian army, however, represents the male portion of the population of the State in the bloom and manly vigour of their age, dwelling in all parts of the country. They live distributed among the rest of the population, in part sharing the house with the citizen, therefore in the most favorable position for infection, and, in part, crowded together in barracks, therefore in a state best adapted to the further distribution of the disease. It has never failed to con-

tribute its share to the increase of other epidemic diseases, when they have appeared; for instance, to typhus and cholera. With reference to the liability to variolous diseases, I here give the not inconsiderable number of cases in the whole army, without regard as to the result of the vaccination. In the whole army there were attacked with variolous diseases,

A.  Year.	B.  Attacked.	C.  Died.	Of the Number re-vaccinated.		
			D. With success.	E. With- out success.	F. No result.
1834 . . . . .	619	38	2	6	30
1835 . . . . .	259	5	—	2	3
1836 . . . . .	130	9	?	?	?
1837 . . . . .	94	3	—	—	3
1838 . . . . .	111	7	—	2	5
1839 . . . . .	89	2	—	—	2
1840 . . . . .	74	2	—	—	2
1841 . . . . .	59	3	—	1	2
1842 . . . . .	99	2	—	1	1
1843 . . . . .	167	3	1	2	—
1844 . . . . .	69	3	1	2	—
1845 . . . . .	30	1	—	1?	—
1846 . . . . .	30	1	—	1	—
1847 . . . . .	5	—	—	—	—
1848 . . . . .	22	1	—	1	—
1849 . . . . .	62	1	—	—	1
1850 . . . . .	176	1	—	—	1
1851 . . . . .	246	3	—	2	1
1852 . . . . .	87	1	—	1	—
Total . . . . .	2428	86	4?	22?	51?
Or if we distribute the 9 who died in 1836 between the columns E and F					
The total would be . . . . .	2428	86	4	25	57

“*Note.*—Of these 86 that have died, at least 5 had had natural smallpox, and on that account were not vaccinated (in the early years no statement thereof was made), while during the same time only 4 died who were successfully re-vaccinated.”



ART. 2.—*Use of Lime-water in the formation of Bread.*

By BARON LIEBIG.

(Annalen der Chemie und Pharmacie, and Chemist, March, 1855.)

To neutralize the deterioration which the gluten of flour undergoes by keeping, bakers add sulphate of copper or alum to the damaged flour. Professor Liebig, however, has conceived the idea of employing lime, in the state of solution, saturated without heat. After having kneaded the flour with water and lime, he adds the yeast, and leaves the dough to itself; the fermentation commences, and is developed as usual; and if we add the remainder of the flour to the fermented dough at the proper time, we obtain, after baking, an excellent, elastic, spongy bread, free from acid, of an agreeable taste, and which is preferred to all other bread after it has been eaten for some time. The proportions of flour and lime-water to be employed are in the ratio of 19 to 5. As the quantity of liquid is not sufficient for converting the flour into dough, it is completed with ordinary water. The quantity of lime contained in the bread is small—160 ounces of lime require more than 300 quarts of water for solution; the lime contained in the bread is scarcely as much as that contained in the seeds of leguminous plants. Professor Liebig remarks that “it may be regarded as a physiological truth, established by experiment, that corn flour is not a perfectly alimentary substance; administered alone, in the state of bread, it does not suffice for sustaining life. From all that we know, this insufficiency is owing to the want of lime, so necessary for the formation of the osseous system. The phosphoric acid likewise required is sufficiently represented in the corn, but lime is less abundant in it than in leguminous plants. This circumstance gives, perhaps, the key to many of the diseases which are observed among prisoners, as well as among children whose diet consists essentially of bread. . . . The yield of bread from flour kneaded with lime-water is more considerable. In my household, 19 pounds of flour, treated without lime-water, rarely give more than 24½ pounds of bread; kneaded with 5 quarts of lime-water, the same quantity of flour produces from 26 pounds 6 ounces to 26 pounds 10 ounces of well-baked bread. Now as, according to Heeren, 19 pounds of flour furnish only 24 pounds 1½ ounce of bread, it may be admitted that the lime-water bread has undergone a real argumentation.”

ART. 3.—*Horseflesh as an article of Food.* By M. G. ST. HILAIRE.

(Gaz. Méd. de Paris, March 10; and Journ. of Health, June, 1855.)

We are far from satisfied with the philosophy of the following remarks. There must have been some reason why the horse was reckoned as unclean in the Levitical code, but apart from this objection, we may naturally ask whether the mere fact that the horse is so valuable as to be kept from the butcher until he is incapacitated from work by disease or old age, is not a strong objection to the use of his flesh as food? The case, however, is thus put:



"Why, then, does not the horse, a large animal, and the most extensively multiplied of our auxiliary quadrupeds, also furnish food? Like all herbivora, the horse produces an eatable flesh, rich in nitrogen, wholesome, and far from disagreeable to the taste.

"Baron de Tott relates, that having been sent as an ambassador from the king of France to the Khan of Tartary, he was in the latter country entertained with an excellent meal of horseflesh." [The use of horseflesh as an article of food by the Tartars is a well-known fact.]

M. Huzard, a veterinary surgeon, relates that in 1789, the Parisians ate horseflesh during three months, and that the public health did not suffer in the least.

Baron Larrey, the celebrated military surgeon, says that horseflesh is very convenient as food for man; it seemed to him especially nutritious. He often saw it used, and with the greatest advantage, by the soldiers and the wounded of the French army. During the siege of Alexandria, in Egypt, in order to overcome the repugnance of the soldiers to this article of diet, he killed his own horses and used them as food.

MM. Cadet, Parmentier, Pariset, and Parent-Duchatelet, have also reported favorably on the qualities of horseflesh.

Our repugnance to horseflesh arises simply from our long having ceased to use it. Anciently, both the horse and the ass were employed as articles of food. The use of horseflesh was at one time general among the inhabitants of the north and west of Europe. The reason from its disuse is thus given by M. Geoffroy St. Hilaire.

"The worshippers of Odin used the horse in sacrifice. When the animal was sacrificed, the flesh was served up on the tables of the priests and of all classes of the population. The eating of horseflesh was thus connected with the rites of the Odin religion, and was a great obstacle to the establishment of Christianity among the people of the north: for, whenever a Scandinavian, even though converted, ate horseflesh, his mind reverted to the recollection of his former faith. Hence at an early period the popes prohibited the use of this article of food. In the eighth century, Pope Gregory III wrote to St. Boniface, archbishop of Mayence, to 'abolish the custom by all possible means, and impose a proper penance on all eaters of horseflesh. They are unclean, and the act is execrable.' His successor, Pope Zacharias, renewed the interdiction.

"Now that the motive of the prohibition issued by the popes has disappeared for many years, the use of horseflesh is being gradually resumed; and it is remarkable, that it is first resumed by those who were the latest to abandon it. Denmark leads the way: in that country, horseflesh is sold publicly under the inspection of the government. For some years, Belgium has followed the example: and recently the Austrian government has authorised the public sale of this article of food.

"It is to be hoped that France will not be the last country to throw off old prejudices. A wholesome, nutritious, economical article of food is lost in France by millions: and at the same time there exist millions of individuals insufficiently fed, and physically and morally

deteriorated. The use of this article of food would regenerate them, and give to the state a class of robust and intelligent servants. If Ireland had been put in possession of this article of food, that country would perhaps not have offered the spectacle of one entire people torn by famine from their ancestral soil.

"In conclusion, M. Geoffroy St. Hilaire observed, that at first horseflesh must be regarded as food for the poor: it is in this character that its utility will be first shown. The rich will use it if they please; and they ought to make use of it for the sake of example, and to prevent the poor from imagining that the use of horseflesh is one of the sad privileges of misery."

### (B) ACUTE DISEASES.

#### ART. 4.—*On the Pathology of Pyrexia.*

By Dr. PARKES, Physician to University College Hospital.

(*Medical Times and Gazette*, June 2, 1855.)

The various influences which seem to be active in fever, and to produce the complex phenomena of this disease by their combined action, are thus enumerated by Dr. Parkes, in one of his recent Gulstonian Lectures before the College of Physicians.

"First of all, we must place the entrance into the blood of a morbid agent, and the alteration of the blood, to a certain extent, under its influence. Perhaps this occurs under the incubative period, when often there is no rise of temperature, no fever that is, and when no appreciable alteration of the general health can be discovered. The nature of the change in the blood is unknown.

"Then, secondly, when the change in the blood has reached a certain point, the nervous system, or rather that part especially connected with nutrition and organic contractility begins to suffer changes in composition, which probably impede or destroy the normal molecular currents. When this occurs, the nervous symptoms of weakness, depression, rigors, and contraction of some parts and vessels, speedily followed by relaxation, mark the stage of invasion.

"Thirdly, and simultaneously, various parts, especially the muscles, and probably some of the organs, deprived in greater or less degree of nervous influence, begin rapidly to disintegrate, and by their disintegration produce supernatural heat.

"Fourthly. This metamorphosis is aided, in most cases, by the condition of the vagus and vasi-motor nerves, which cause increased action of the heart, and dilation of the vessels.

"Fifthly. The contamination of the blood, already produced by the morbid agent, is increased by the check which the normal extravascular currents experience, by the pouring into the blood of the rapidly disintegrating tissues, and by the continued action of the morbid agent, which in almost all cases appears to act more rapidly and more powerfully in blood rendered impure in any way, either as shown by Dr. Carpenter by retention of excretions, absorption of

septic substances, or, as in fever, by the too rapid metamorphosis of tissue.

“Sixthly. The various organs suffer (apart altogether from specific changes), and must, one would think, produce increased deterioration of the blood. Thus the lungs are congested in so many cases that we can scarcely suppose proper aeration to go on; the liver would seem, from Frerichs’ observations to be, in some cases at any rate, in a most abnormal condition, and to produce compounds such as leucin, unknown in health—and the spleen in many fevers, if not in all, enlarges, (in persons of a certain age,) and is congested, possibly even to extravasation.

“But to these complex conditions another must yet be added; food is almost entirely withdrawn, and the various alkaline and neutral salts, unless supplied in the form of medicines, no longer pass into the system. But as in the excretions these salts are continually passing out, and are not restored, there must at last in fevers be a most unusual disproportion between the organic and the inorganic constituents of the frame. The blood will show this the latest, for it seems to maintain its composition, as far as the salts are concerned, with great tenacity; and it probably takes from the organs the ingredients it loses by the excretions. The exact influence of this loss of salts is not certain. The blood seems certainly to become less alkaline, and it is by no means improbable that this may render oxidation less complete than it should be, and thus cause some of those instances of retention of effete materials to which I have formerly referred.

“Thus the blood is contaminated by primary action of the agent, by the products of metamorphosis of tissue, by the loss of the salts, and by the altered action of organs; the nervous system is, therefore, day by day constantly more affected, and reacts still more on metamorphosis, the heat increases, the heart’s action still quickens, and the fever reaches its acme.”

Dr. Parkes lays great stress upon the influence of the nervous system in the causation of fever, and we think there is great cogency in his arguments. Certainly there is great clearness and force. In order that full justice be done to the writer, however, it is necessary to read all that he has written on the subject, and for this reason we will hope that before long we may have an opportunity of seeing these lectures on Pyrexia in a separate and extended form,—for no one can read what is written without wishing to read more. Speaking, then, of the influence of the nervous system, and premising that its investigation is vague and unsatisfactory, Dr. Parkes proceeds:

“The tests we have to employ are the symptoms of the diseased body, and the only corrective is our physiological knowledge of the healthy working of the nerves. But the meaning of symptoms is often hard to understand, and the physiology of the nerves is yet almost a virgin soil, bearing often only enigmatical fruit.

“The time will come when the alterations in the nerves may be tested during life, and be recognized after death, by the electrical multiplier; it may be, possibly by muco-chemistry. But till that time arrives it becomes us to advance everything with caution, and to regard even our apparently most certain conclusions as only provisional.



“And yet, at the same time, if we are ever thoroughly to comprehend disease, the condition of the nervous system must be understood. The part it plays in every malady is no insignificant one. It modifies, controls, intensifies, cures, and kills. The blood itself does not so quickly carry to other structures the impression of the suffering part, and the old doctrine of sympathies has its side of truth. We must then search, if we are ever to advance, and conjecture, if certainty is ever to be attained.

“The arguments which can be now brought forward to prove the influence of the nerves in febrile affections impress different minds with different degrees of force. Altogether, however, it would seem, if we may judge both from the older and recent writers, that the essential participation of the nervous system is now doubted by few, and the chief subject of debate is the extent and manner of this participation. Few of the masters of our profession have left the subject untouched, and it would be curious and interesting to review the various opinions and hypotheses which have been advanced. This cannot, however, now be done, and I must proceed to a very brief statement of the various reasons which seem to show at any rate that the nerves are greatly affected, and perhaps to bear out Virchow's statement that this affection is the cause of the other more obvious symptoms.

“The most striking phenomena of fever are the augmented metamorphosis and the preternatural heat. Now, over normal metamorphosis and normal heat, the nervous system seems to rule paramount. The influence of the nerves on heat, as developed in muscular action, has been shown by Helmholtz; their regulating power over secretion has been proved by Ludwig; their control over nutritive processes has, if the experiments of Axmann are to be trusted, been at last experimentally established. At the very first step we have at once this question, Is it likely that the system which plays so great a part in normal heat, secretion, and nutrition is inoperative and inert when all these processes are deranged?

“To this question a partial answer is given at once by the beautiful experiment of Bernard, which has conclusively shown that artificial disease of the nerves at any rate will alter both tissue-change and normal heat. It would appear as if the tissues at once began to suffer oxidation; as if, in fact, it were the nervous power which had previously prevented from destruction. The older and recent experiments in section of the nerves show the same fact. Thus, when, by section or extirpation, the controlling effect of the sympathetic in the neck is taken away, there occur at once hyperæmia and local development of heat, far exceeding the heat of the blood, in the parts not deprived of nervous influence. By the side of these physiological arguments we can place others derived from the early symptoms of fever, which seem inexplicable unless the participation of the nerves is admitted; and to these again we can add more or less cogent evidence afforded during the course and at the termination of these diseases.

“Among the very earliest symptoms of febrile affections are the remarkable depression, apathy, exhaustion, and debility which were much and justly insisted on by Cullen. It is indeed possible that these may be the effects of a general nutritive failure, in which the nervous sys-



tem merely participates in an equal, but in no higher a degree, than other parts. If there were no other evidence of nervous affection, this argument might be a good one, although the nervous symptoms are certainly unusually prominent; but these acquire significance from being placed in juxtaposition with others.

“Another very early symptom of fever is one which seems to indicate most decidedly a more than simple co-affection of the nervous system with other parts. I allude, of course, to the shiverings, the contraction of the superficial vessels, and of the skin. At this time, if not before, the tissue metamorphosis is most decidedly augmenting, for the heat of the blood is rising, as shown by these observations. We have at this time the remarkable subjective sensation of cold, and the rigors, which stand in such striking contrast to the augmenting heat. The explanation of this has been already alluded to, as being given probably by the nerves of the skin and cutaneous vessels, which transmit to the sensorium the condition of peripheral parts. If this be the case, we have the anomaly, pointed out by some German writer—Henle, I think—of the impression of cold being transmitted from distant nerves along the trunks of nerves, which, lying deeply, and being fed by vessels which are not contracted, must be hotter than usual, although they thus transmit the sensation of cold.

“Another very early symptom of fever finds its readiest, perhaps its only, explanation in some condition of the nerves. I refer to the increased rapidity of the heart's action, and to the relaxation of the vessels which soon follows the stage of contraction just referred to, or occurs without it. The increased cardiac action occurring at too early a period to permit us to refer it to altered nutrition of the fibres, or to action of a depraved blood in the endocardium, and reflex action, the hypothesis which refers it to a diseased condition of the vagus is much more probable than either of these propositions, for the vagus is the nerve which regulates the cardiac movements. The experiment of Weber seems to strengthen this supposition, for section of the vagus quickens at once the action of the heart, and the transmission of electrical currents, the nearest approach to the normal currents which we possess, at once lessens again the action. Volkmann again finds that section of the vagus produces an increased lateral pressure in the vessels. Therefore two of the most striking phenomena of fever, the increased cardiac action, and the relaxation of the vessels can be artificially produced at will, by interfering with the nervous currents.

“The affection of the vagus has appeared to some so certain, that it has been attempted to prove it to be the essential and proper disease, from which all other febrile symptoms arise. Thus the heart's action being quickened, and the vessels being relaxed, increased circulation, general hyperæmia, and preternatural heat, would seem to be the necessary consequences. But this opinion does not bear examination; for there are cases of fever without quickened circulation, and when there is quickened circulation, it bears no relation whatever to the abnormal heat. On this point much evidence has been published, and I have myself accumulated proof upon proof, which I think it unnecessary to adduce here, that the quickened circulation in various febrile diseases, and the dilatation of the vascular system, as far as

this can be judged of by the pulse, are entirely unconnected with, and independent of, febrile heat. And a physiological argument seems to settle the question that, besides hyperæmia, there must be increased tissue-change to account for the heat; for, in Bernard's experiment, the heat of the side of the heart which was deprived of nervous influence was greater than that of the blood; and though there was enormous hyperæmia, this, by itself, could never raise the heat above the temperature of the blood at large.

"The occasional absence of this increased cardiac action shows that, when it does occur, it is not owing to diseased blood, for this must exist in all cases; and this is an argument the more for locating the cause in the vagus.

"Another very early symptom of fever seems to find its most reasonable explanation in implication of the vagi. I refer to congestion of the lungs, which is so common in almost all febrile diseases as to oblige us to connect it rather with the general febrile state than with any specific disease. It has been lately shown by Woilley that, at the commencement of all acute diseases, in typhoid and rheumatic fever, in ague in some cases, in variola, in scarlatina, measles, and erysipelas, in acute inflammations of the heart, congestion of the lungs is so common, that it is discovered in 80·5 per cent., and is announced by unequivocal physical signs.

"The pulmonary congestion of a later period in most of these diseases in various degrees is a fact which has been long known. To what, now, is this pulmonary congestion to be referred? To altered blood refusing to pass through the pulmonary capillaries, to perverted contraction of those capillaries, or to some alteration in the circulation consequent upon altered innervation? It must be confessed that we have few facts to guide us; but when we remember that section of the vagi produces (of course in a still higher degree) the same condition of congestion and œdema of the lungs, and that there is reason to believe, from the condition of the heart, that the regulating nervous currents of the vagi are altered, it seems most reasonable to refer the pulmonary congestion to the same cause as the augmented cardiac action.

"It may be possible, as observed by Virchow, to trace the effect of alteration of the pneumogastrics, or of the nerves connected with them, still further, even to the digestive organs, and to ascribe some of the early symptoms of anorexia and nausea to this cause, but it is unnecessary to push this argument further.\*

\* Since this lecture was delivered, my friend Dr. Radcliffe Hall has given me the important information that he finds congestion in the hepatic veins to be also an invariable sequence (48 experiments) of section of the two vagi, and to be a frequent result of the section of one vagus. The congestion is confined to the hepatic vein; the portal system was not engorged. Dr. Hall writes;—"My experiments were not published; they consisted of section through the vagus, partial section, partial burning with cautery, and partial ligaturing of the large ganglion of the vagus, all with the idea of setting up irritation in the ganglionic structure, and observing the results. I was disappointed to find that there was great difficulty in producing irritation, merely owing to the effusion of lymph consolidating the tissue of the ganglion and glueing it down to the parts adjoining, virtually occasioning complete constriction of the vagus through its ganglion, with the usual results of complete ligaturing of the nerve in any part of its

"Pursuing now the fever into its developed period, we have a remarkable and very frequent phenomenon, viz., the evident periodicity which attends many cases, which can be accounted for only by acknowledging, not merely that the nerves are implicated, but that this affection is of that kind which subordinates and controls the other symptoms of the case. The symptoms in ague, and the wonderful periodicity which was shown by the late Dr. Graves to govern even the remote outbreaks of attacks are the most striking examples; but the course of all the febrile affections, and even of the acute inflammations, indicate the same thing.

"I shall not venture, and do not further allude here to the subject of critical days; for it would require more time than could be given me to do justice to it; but I must remark that the experiments of Traube, and others, prove that this ancient doctrine must not be thrown aside, as an hypothesis born from the old mystery of numbers, or as a mere dream springing from the wild imagination of the East, and imported into Greece. There is much, though it is not easy to say how much, truth in critical days; and, if so, the nerves must surely play the principal part in their production.

"Again, in the course of fevers, the secretions are very much altered in quantity, and possibly, though of this we know nothing, in quality. Now, the nervous system certainly guides and controls the flow of secretion.

"Then, passing from the course to the end of fevers, we may observe that the occasional sudden termination in some cases, and the way in which some fevers, as ague, are readily cured by a few grains of medicine, which can scarcely be supposed to alter the constitution of the blood, but the action of which on the nerves is shown by other facts, are again arguments that, in these cases, the febrile symptoms are under nervous control.

"I must allude here to one most enigmatical mode of termination of fever, which possibly may be connected with the nerves. It is well known that in most severe fevers there occur instances in which patients die in the early stage, from an unknown cause. It is usually said that the pyrexia itself kills them, independent, as it were, of the specific disease. In such cases, no sufficient anatomical condition is found to account for death. In fact, the various chemical products, which, acting on different tissues, constitute the anatomical signs of the specific fevers, are not formed: there is fatal smallpox, or scarlatina, without eruption, fatal typhus, without a rash, fatal typhoid fever with very slight Peyerian deposit. How, then, do such patients die? It may be that there is some alteration in the blood, so profound, as to render life impossible; and, in proof of this, it appears that purpuric spots, blebs filled with bloody serum from dissolved red

cervical course, which are the same as those induced by section. Until the symptoms of such obstruction of nerve came on, very little physiological effect of any kind ensued. Gradually, as the lymph-compression of ganglion proceeded, slow, deep inspirations and dyspnoea, and the usual sequel of pulmonary congestion and effusion supervened, and, sooner or later, death, provided both nerves had been subjected to experiment. There was no exception among the animals examined to the fact of hepatic venous congestion, I believe."



particles, weeping of such red fluid from mucous surfaces, and such like evidence of a destroyed blood, are generally seen in these cases.

"But, besides this, may it not be that in these cases there is profound nervous lesion also? There is extraordinary prostration, a galloping and early-failing pulse, and an excessively rapid respiration, to account for which there is only pulmonary congestion. The mind, it is true, may be perfectly clear in these cases; but that only proves that one special part of the nervous system is untouched.

"Leaving, however, this doubtful point, the results of the argument in proof of the implication of the nerves may be thus summed up. These are: 1. The general physiological law that nerves regulate the metamorphosis of tissue and the production of heat, which are both altered in fevers; 2. Experiments on the sympathetic and the vagus, the results of which simulate, so to speak, or are identical with, the febrile phenomena; 3. Various symptoms which announce, accompany, or terminate fevers; 4. The effects of certain remedies.

"Whether these various arguments will appear sufficient to any one will, I think, very much depend upon the weight which he attaches to the physiological and the experimental part of the argument. Those who are imbued with a sense of the constant and necessary action of the nerves on nutrition will find their opinions give strength to the otherwise comparatively weak arguments which are drawn from the symptoms and the course of fevers.

"Against the view that the nerves are especially and essentially implicated, we have the argument that no decided experimental proof has yet been given of abnormal innervation; but then, in the present state of physics and micro-chemistry, this argument is really worth little.

"If there be perverted innervation as a necessary part of fever, in what does it consist?

"Two opinions only need be noticed: one advanced some years ago by Henle, that there is irritation of the nerves; the other, of more recent date, and founded upon recent experiments, that there is partial paralysis of the nerves, or rather of certain of the nerves.

"In favour of this last opinion we have the following facts:—

"Wherever, in experiments on nerves, the phenomena are like those of fever—viz., augmented circulation, relaxation of vessels, perverted nutrition, and abnormal heat—the state is one in which the nerve-currents are interrupted either by extirpation, section, ligature, or chemical destruction of the nerves. On the other hand, irritation of nerves by electrical currents produce phenomena different from those of fever. Thus the vagus is cut, the heart beats rapidly; when the cut vagus is irritated (so to speak) by galvanism, the heart beats again slowly; when the sympathetic of the neck is cut, the vessels of the side of the head enlarge, the part grows hot; when a galvanic stream is passed through the nerve, as in Dr. Waller's interesting experiment, the vessels contract, and the heat disappears.

"Other experiments, as already said, lead us to infer that section of the sympathetic or of other nerves connected with nutrition is always followed by disintegration of tissue, and perhaps even by final death of the part.



“Coupling these facts with the early symptoms of prostration and languor, we may conclude that the state of the nerves is one rather of exhaustion and paralysis, than of irritation and excitement.”

ART. 5.—*On the Pseudo-pneumonia of Typhus, &c.*

By Professor STOKES.

(*Medical Times and Gazette*, May 26, 1855.)

In a clinical lecture from which we take the following remarks, Dr. Stokes directs attention to those typhoid affections of the lung which may be confounded with pneumonia. “These conditions,” the speaker says, “may easily be confounded with pneumonia. They have all the physical and other signs, except it be the sign of tympanitic resonance over the diseased lung. There is, however, a circumstance in connexion with the resolution of these typhoid or typhous diseases of the lung, different from what is commonly observed in sthenic pneumonia. You know that the true inflammatory hepatization rarely disappears in a sudden manner. It subsides gradually, and the transition-state between dulness and clearness on percussion is generally marked by the “crepitus redux.” In the cases before us, however, and especially where the disease is secondary to typhus fever, the resolution, as I have before stated to you, is often singularly rapid, and is often unattended by the crepitus of resolution. If, then, you consider the state of solidification simply, we find it on the one hand to form without the crepitus of the first stage of pneumonia, and on the other to disappear rapidly, and without the rule of resolution. Thus we are permitted, as it were, to witness the silent and spontaneous development and retrocession of one of the secondary diseases of typhus.

“This change from the state of consolidation to that of permeability to air, this rapid change, unattended by the crepitus and resolution, probably shows that the real disease was one unconnected with inflammation, either as a primary or a reactive condition.

“You will remember that I suggested to you that some of the cases which have been described as typhoid pneumonia, might be held as examples of an aborted typhus. These were characterised by early consolidation, early disappearance of the typhous state, and a rapid, and often spontaneous subsidence of the local disease. I cannot help thinking that between such cases, and those in which the general disease runs its usual course, there is another class or category of cases in which the progress of the merely pulmonary disease is marked, more or less, by signs of irritation or inflammation of the lung, which inflammation or irritation is either reactive or specific, or both reactive and specific. And I apprehend that these cases which, as it were, float between the aborted and the perfect typhus, are much more numerous than might be supposed; and in such instances the case is often treated throughout, without a suspicion of its being really an example of typhous disease having been entertained.

“What has been now said should impress on your minds that rule in practice which I have so often urged upon you, namely, that the

rules of diagnosis of local inflammatory disease which are good in ordinary cases, lose their value in a great measure when the patient has typhus fever. This was long ago proved by the researches of Louis on the condition of the brain in fever, and it was the non-recognition of this fact which constituted one of the greatest errors in the system of Broussais. I have told you, that if you gained nothing during the session but the knowledge and full appreciation of this great principle, your time would have been well spent. How many cases have we not had of headache, delirium, watchfulness, or its opposite, coma,—yet without encephalitis; and so it is with the remaining cavities—symptoms of functional alteration are met with in connexion with the cerebral, pulmonary, circulating, and digestive systems in fever. They may or may not be attended by organic change, and that organic change, when it does exist, is not necessarily inflammation; and we cannot, I believe, lay down any satisfactory rule of diagnosis which would show, that in one case of local functional disturbance there was organic change, and in another that there was not. But this much we do know, that those groups of symptoms which are diagnostic of local inflammation in a case which is not fever, cease to be so when they occur in a case of typhus. Let this principle be ever present to your minds, for it is impossible to exaggerate its value. Long ago it was acted on empirically by the best physicians, who refused to adopt antiphlogistic measures in treating the local symptoms in typhus, and who employed stimulants irrespectively of them, when the general condition seemed to demand such treatment. It now comes before you as the result of an extended and accurate pathological investigation, and the study of the pulmonary phenomena, as we have seen, enables us to go a step further, and to declare that not only are the symptoms of local irritation doubtful or illusive; but that even the physical signs of a pneumonia, when occurring in a case of typhus, are not to be taken as proof that a local inflammation has occurred.

“If these things be true so far as our typhus is concerned, it would appear probable, that in other acute diseases, under the influence of a law of periodicity, and, perhaps, in many that arise from the operation of an introduced poison, the same circumstances may be found, so that we might apply to a much larger circle of diseases those principles as to the secondary local affections, which appear applicable to typhus fever.”

ART. 6.—*On the Deafness connected with Fevers (typhus, smallpox, &c).*  
By M. TRIGNET.

(*Archiv Belges de Méd.*, Juil. 1855; and *Gaz. Méd. de Paris*, Sept. 22, 1854.)

M. Trignet refers this symptom to inflammation of the internal ear, and his treatment is directed to the prevention of the accumulation of pus in the auditory cavities, and of the ruinous consequences of this accumulation. In order to this, he first tries the effect of leeching, cupping, or blisters in the neighbourhood of the external ear. If this treatment fails, he catheterizes the Eustachian tube—for

this tube is obliterated from the first by the tumefaction of its mucous membrane,—and injects through the catheter certain emollient, anodyne, or slightly irritating solutions, according to circumstances. And lastly, if this treatment fails, he perforates the membrana tympani, and introduces the injection through the opening. Under no circumstance must the internal ear be allowed to remain distended with matter longer than can be helped.

ART. 7.—*On certain spasmodic complications occurring in Typhoid Fever.* By M. ARAN.

(Gaz. Hebdom. de Méd. et Chir., May 4, 1855.)

This is a complication which has been only noticed occasionally, and which may very easily, without great care, lead to serious error both in diagnosis and treatment. Similar symptoms have prevailed at different times in certain localities, and not always in connexion with typhoid fever, but comparatively little attention has been paid to them.

M. Aran met with twelve instances of this complication during the first four months of the present year, in the Hôpital St. Antoine, at Paris, and all of them were in cases of typhoid fever. In eleven of these instances the symptoms made their appearance late in the history of the fever—from the twelfth or fourteenth to the twentieth or twenty-fourth day. Their accession was marked by tingling in the limbs. After this the patient was seized with acute and painful cramps, these cramps being generally limited to the hands and arms, but extending occasionally to the inferior extremities, and even to the trunk and jaws, causing opisthotonus, trismus, and great difficulty in speaking and swallowing. These attacks, as a rule, lasted for several hours, and recurred for several days; and all this while the fever progressed through its ordinary course without any perceptible change. Pain was caused by any attempt to straighten the contracted parts, but this straightening was followed by immediate relief, so that the patient wished the attempt to be made. On the other hand the cramps might readily be provoked by squeezing the limbs. The spasm itself was very tremulous in its character.

Of the twelve patients mentioned, three died, evidently from typhoid fever. Their spinal cords were not examined; but M. Aran thinks that the character of the spasm was such as not to allow us to suppose that any important alteration would have been found if the examination had been made.

ART. 8.—*The state of the Respiration in Fever.*  
By Dr. SAMUEL WILKS.

(Medical Times and Gazette, May 12, 1855.)

The result of Dr. Wilks's observations is that the number of respirations is increased in all febrile affections (care being taken to exclude all those instances where there was the slightest symptom of



pulmonary complication or obstruction), and that this increase is quite irrespective of the pulsations of the heart. He says:

“For although the action of the heart and lungs is no doubt, to a certain extent, associated, and the amount of work done by one organ is a measure of that done by the other; as, for example, when each is increased during any violent exertion; yet this is by no means invariably the case. There are times, undoubtedly, when the blood, containing more effete matter than it does at others, requires for its purification an increased action of the lungs, and yet the number of cardiac contractions may be of the ordinary amount. I believe this occurs in fever, and accounts for the phenomena which are observed—the constant dissociated action of the heart and lungs. In looking over a large number of reports, I find, as a rule, that the respiration continued high as long as the fever lasted, while the pulse was often at the natural standard, or even below it.

“Taking the ratio of the respirations to the pulse in health to be 1 to  $5\frac{1}{2}$  or 6, *i. e.*, reckoning the former at 12-14 per minute, and the latter at 70, and then looking at continued fever, we find the average pulse in that disease to be 100, and the respiration 25-30 per minute, making the ratio 1 to 3, instead of 1 to 6. Often even during the course of fever the pulse may be descending, while the respiration remains high. Thus, for example, a woman with typhus and a mulberry rash, and having no chest or abdominal symptoms, had a pulse 116 and the respiration 36 in the minute; the former soon became 100 and the latter 32; the pulse then sank to 90, and afterwards still lower to 52, while the respiration had only reached 26; the skin was still hot and dry, and the pulse descended still farther to 42, while the respiration was 22. The respiration is here seen as much above the standard number of health as the pulse is below it. Perspiration broke out, and the patient convalesced, and at the same time the pulse rose and the respiration fell until each had reached its natural number. In all cases of fever the pulse does not descend so low, but constantly in typhus, towards the height of the disorder, the pulse may be at 70 while the respiration is, as a rule, double that of health. The same facts as above stated may be found in scarlatina, measles, and other febrile diseases; but as it may be objected that a congestion of the lungs in fever and the exanthemata may be sufficient to account for the phenomena, we may take rheumatism, and selecting cases where no pulmonic or cardiac complication existed, we still find that while the pulse in number was only half as many again as in health the respiration was doubled or trebled. In cases where lemon-juice was given this difference was more than usually marked, for, as is well known, this drug is often observed to have a direct influence in lowering the action of the heart. In one case the pulse was 120 and the respiration 36; in three days the former was 70 and the latter 32; after three days more the pulse was still 70 and the respiration had reached 24. Thus the effects of the remedies had been to depress the heart's action, while the respiration was only lowered as the disease more slowly departed. In another instance of a lad where the pulse was 110 and the respiration 40, on the following day the former was 100 and the latter 32: in three days the pulse had fallen to 76 while



the respiration was still 32, and during convalescence the pulse remained steady while the respiration gradually subsided to 17.

"The fact then being that the number of respirations in all febrile diseases is increased, I assume that this is indicative of a positive increase of function of the lungs, that the blood comes to the lungs loaded with an increased amount of effete matter to be eliminated, that the *besoin de respirer* is more felt, and that the function is for the time augmented."

ART. 9.—*Some points in the treatment of Pyrexia.*

By Dr. PARKES, Physician to University College Hospital.

(*Medical Times and Gazette*, June 9, 1855.)

Dr. Parkes does not pretend to enter fully into the subject of treatment. He merely hints at certain points, and out of these we select the remarks upon the use of alkaline salts and coffee as especially worthy of attention.

"To ensure proper excretion in fevers is a much more difficult thing than to reduce temperature. It is, perhaps, best performed by constantly supplying to the system a due supply of alkaline salts, which are not now given in the food. The chloride of sodium, we know from the experiments of Bischoff, aids the formation and the elimination of urea. Whether it has the same effect in fevers has yet to be determined. The alkaline salts of potash, and probably those of soda, do certainly aid the elimination of urea and sulphuric acid in some febrile cases, in pneumonia, rheumatism, variola, and typhoid fever. Perhaps they so act in all cases. The nitrate of potash seems also to aid elimination in some febrile cases, though it does not necessarily do so in health.

"I have observed a singular fact in several febrile diseases, viz., that, at the first employment of a saline remedy, such as nitrate of potash, which is not a natural constituent of the frame, or iodide of potassium, there is sometimes for a day or two a marked lessening of excretion, as if the presence of this foreign substance had interfered for the time with the chemical processes then going on; afterwards elimination increases, as if the system had accommodated itself, so to speak, to the remedy."

\* \* \* \* \*

Again:

"Much attention has been lately directed to the powerful effects of coffee, and of tea, and of cocoa, especially of coffee, in lessening the elimination of urea. The late observations of Dr. Julius Lehmann have shown that coffee, in health, has two powerful actions; it increases the nervous energy, and protracts the metamorphosis of tissue. He thinks there are antagonistic effects, but they may possibly be simply cause and effect. Lehmann only determined the urea and the phosphoric acid, which he found both diminished.

"It would be very interesting to know if coffee has the same effect in the febrile body as in health.

"I have made one experiment on the point, and I think none has

yet been made known as having been performed by others. Although my observation is incomplete, I shall venture to give it.

"In a case of typhoid fever, which was so far favorable for the remedy as, though perfectly well marked, there was no diarrhœa or sweating, coffee was administered. During twenty-four hours the patient took  $\text{zvj}$  of infusion of colonial coffee (of course without sugar or milk). The coffee contains 207.888 grains of solid matter; there were only traces of chlorine; there was 1.738 grain of phosphoric, and 1.055 grain of sulphuric acid in the whole quantity.

"In the next twenty-four hours he took another  $\text{zvj}$  of coffee, containing 197.328 grains of solids. In the third twenty-four hours,  $\text{zvj}$  of coffee were given, which contained only 34.89 grains of solids.

"Unfortunately, before the experiment could be commenced, the temperature was beginning to decline, and the urea, the sulphuric, and the phosphoric acids of the urine were gradually diminishing.

"Thus, for three days during the height of the fever, about the eighteenth or twentieth day, the urea amounted to 429 grains in each twenty-four hours; the sulphuric acid amounted to 28.537 grains, and the phosphoric acid to 19.275 grains. On the following days, before the coffee was given, the fever was declining, the urea fell to 326.04 grains, viz., 103 grains less; the sulphuric acid fell to 23.34 grains, or 5 grains less; the phosphoric acid fell to 17.446, or 2 grains less.

"The coffee was then given, the diet and all other circumstances being unaltered. During the three days it was taken, and on the following day over which its action may be presumed to extend, the urea averaged, in each twenty-four hours, 277.04 grains; the sulphuric acid averaged 16.502 grains, and the phosphoric acid amounted to 9.227 grains. It thus appears that the lessened amount of urea was not more during the use of coffee than could be accounted for by the decline in the fever; but the fall in the sulphuric, and especially in the phosphoric, acid was extraordinary; deducting the grain of sulphuric acid added in the coffee, the amount of the acid was  $15\frac{1}{2}$  grains as against  $28\frac{1}{2}$  and  $23\frac{1}{2}$ ; the phosphoric acid did not exceed 8 grains as against 19 and 17.

"After the coffee was left off the urea and sulphuric acid soon increased again; the phosphoric acid fell still more the next day (to 5.5 grains), and then increased again.

"There appeared to be no modifying circumstances to interfere with the action of the coffee, and though the effect on the urea was not certain, yet, as it increased again when the coffee was taken away, and as the sulphuric acid was so diminished, it is probable that metamorphosis was checked. The lessened phosphoric might depend on lessened amount of disintegration of the nervous tissue.

"Certainly there seems good reason to try the effect of coffee in severe cases of fever with delirium and rapid wasting."\*

\* Since this lecture was delivered I have had the opportunity of trying it in a very severe and ultimately fatal case (from perforation). The urea did not appear to be diminished.

ART. 10.—*On the therapeutical employment of Electricity in Cholera.*  
By M. SCHULTZ.

(*Weiner Wochenschrift*, No. 3 and 4, 1855.)<sup>1</sup>

The object of M. Schultz, in this memoir, is to call attention to the possible good which may attend upon the employment of electricity in this disease, and with this view he communicates the results of his own experience up to the present time. These, then, are the results which he has obtained in a series of experiments with the ordinary induction-coil. The passage of the current caused more pain than it did in the healthy state. The passage of the current through the flexors caused persistent cramp, but if cramps already existed in these muscles they were relaxed by passing the current through the extensors. On introducing one pole into the rectum, and applying the other to the pit of the stomach considerable pain was caused in the bowels, if the conductor over the epigastrium was moistened, but if only dry, there was an agreeable feeling of warmth in place of pain, and the patient for the time ceased to have involuntary evacuations, though the character and amount of the evacuations were not changed. When one pole was placed at the front of the chest, and the other upon the abdomen, the pulse acquired force, and the cyanosis became less marked. The passage of the current along the course of the inferior laryngeal nerves had no appreciable effect in recalling the voice, though in two instances in which the experiment was tried, the patient in recovering seemed to regain his voice more rapidly than in cases where the experiment was not tried.

More evidence is required on this subject, and it is with a view to elicit this that the present memoir is published.

ART. 11.—*A prophylactic in Yellow Fever.* By M. HUMBOLDT.

(*Lancet*, Aug. 18, 1855.)

A nephew of the celebrated Humboldt is now creating much sensation at Cuba by the discovery of a prophylactic against yellow fever. He noticed that the sting of a small reptile, in Mexico, caused symptoms much resembling an attack of yellow fever, and he rendered the fact quite certain by a series of inoculations on a number of dogs. M. Humboldt then thought that, by exciting the symptoms in a mitigated form, he might perhaps preserve persons from an actual attack of yellow fever. He therefore took a piece of sheep's liver, weighing one ounce, and had it bitten by six of the little vipers; he allowed the piece of liver to putrefy, and inoculated some dogs with the expressed fluid. By a few inoculations, febrile symptoms were excited; but they soon went off, and the punctured spots presented nothing particular. The experiments were now extended to human beings, the first being men under sentence of death. The patients were twelve in number, and were inoculated by four punctures on the arm; they were in a few hours seized with headache and severe pain in the back, and for the next few days had regular attacks of a kind of ague during



several hours. Afterwards, however, health became quite restored. More than 200 persons, either galley-slaves or Europeans newly arrived at Vera Cruz, were inoculated, and escaped yellow fever for the next three years. In 1850, 1851, and 1852, the persons inoculated were no less than 1438, and amongst these only seven had the yellow fever: they recovered. At New Orleans M. Humboldt inoculated 286 newly arrived Irishmen and Americans from the northern states, and none took the yellow fever during a very severe epidemic of the disease.

Experiments have latterly, as we stated above, been made at Cuba, where yellow fever breaks out very frequently, and the first trials were instituted amongst the military of the garrison. Four regimental surgeons submitted to inoculation before the men, and had but slight symptoms; two hundred of the latter followed, and the authorities have sanctioned the founding of an establishment specially intended for prophylactic inoculations. We extract the above from 'L'Union Médicale' and the 'Revue Thérapeutique du Midi,' which latter journal obtained the information from a paper presented by M. Humboldt to the Academy of Sciences of Havannah.

The editor of the 'Revue' remarks very justly that the venom of the viper is very likely to be destroyed by the putrefaction of the sheep's liver, and that the results obtained might, after all, be owing merely to a dynamic modification occasioned by the inoculation of putrid matter.

#### ART. 12.—*On the treatment of Erysipelas.*

By Dr. TODD, F.R.S., Physician to King's College Hospital.

(*Medical Times and Gazette*, July 14, 1855.)

Dr. Todd arranges the various cases of erysipelas into five groups, and he directs attention to these before speaking upon the treatment particularly. (The remarks occur in a clinical lecture.)

"*a.* There are certain cases of erysipelas which get well of themselves, and these are generally examples of the disease in a slight form, affecting the head and face only, or some other limited portion of the skin. Usually in the course of two or three days, especially if they are kept in a comfortable place, and have a little beef-tea, patients suffering from attacks of this intensity recover, and this, too, in some cases, despite of a lowering treatment, and of the use of such remedies as tartar emetic.

"*β.* In a second set of cases of this disease, the very opposite of the first class, the patient dies downright, if I may use the expression; he sinks rapidly, do what you will. This mode of termination is common to erysipelas, with most of the other diseases which are due to the influence of the poison, whether it be generated in the body, or be of atmospheric origin. Thus, in a considerable proportion of cases of cholera, it is perfectly in vain to attach very much importance to anything in the way of treatment, for the patient is dead almost before the case comes fairly under observation: and, in such instances, I doubt that any plan of treatment ever will avail, because the morbid phenomena are of such rapid accession, almost as rapid as if the



patient had taken a large dose of arsenic or of prussic acid. The same kind of thing also occasionally happens in typhus fever, the patient being killed, within a very few days, or even hours, of the commencement of the attack; and so, likewise, in all the exanthemata—small-pox, measles, scarlet fever, &c.,—all treatment often utterly fails, and death occurs during the first twenty-four or forty-eight hours of the illness.

“Hence, then, it becomes necessary in all endeavours to estimate the value of any particular plan of treatment in erysipelas, carefully to eliminate from the data upon which conclusions are to be founded these two classes of cases, viz., first, those which would get well of themselves; and, second, those which defy every attempt at treatment.

“γ. The third class of cases of erysipelas comprises those which recover under a suitable treatment, but in which there is a marked tendency to death, but which there is good reason to believe would terminate fatally if left to themselves.

“δ. The fourth group consists of those cases which pass through the early stages of the malady more or less favorably, but which then exhibit the secondary phenomena of the disease. In this class complete recovery may take place, or death may result from the extreme exhaustion which is frequently induced by the extension of the suppurative process, and by its duration.

“ε. In the fifth and last class, not only do the secondary phenomena of the disease manifest themselves, but, by some means or other, some morbid material finds its way into the circulation, in effect of which formations of pus take place in various parts of the body, and the patient dies of purulent infection, or pyæmia, as it is called.

“The treatment for erysipelas which I have for many years past adopted, is the stimulating and supporting plan; and this I would, from a long experience, recommend to you, under the conviction that it is the best adapted to save life, and check the progress of the disease; and that under it you will seldom have to deal with the secondary phenomena of the malady.

“The treatment consists in the free and steady administration of food and stimulants; such as beef-tea, and some form of alcohol, brandy by preference, in regulated quantities, at stated and short intervals; and if drugs are needed, ammonia, bark, and chloric æther, in forms most agreeable or least offensive to the stomach. The beef-tea and brandy should be given at stated times, in small doses, two or three ounces of the former, and from two drachms to half an ounce or an ounce of the latter, slightly diluted with water. Two different forms of alcoholic fluid should not be given at the same time, such as wine and brandy, or beer and brandy, or gin and brandy; and for other nourishment it is desirable to observe the same rule, as far as possible. You must attend closely to the digestive power of your patient, and be careful to avoid exciting dyspeptic symptoms, such as nausea, sickness, hiccough, flatulence, by giving too much at one time, or by too great variety of stimulant or food.

“Sometimes in the course of an attack of erysipelas, the patient

may become delirious, or he may fall into a state of coma. When this occurs, some authors would tell you that the erysipelas is inducing inflammation of the membranes of the brain. These notions are now, however, almost entirely exploded, and there is ample evidence that, if death takes place while the patient is in either of these conditions, the cerebral meninges are found, upon post-mortem examination, to all appearance perfectly healthy, or, if there be anything amiss with them, it is that the vessels of the pia mater contain rather less blood than they ought to do, and that none of the products of an inflammatory process can be detected.

"It is during the first fourteen days of the illness, that these formidable symptoms are most apt to occur; hence the necessity of beginning early, from the first, with the supporting and stimulating treatment, which you will find a preventive both of delirium and coma. The lower you keep your patient, the greater will be the tendency to delirium or coma, and the more violent or profound will either be; and the development of either is an indication for pressing that treatment in the same or greater doses. Sometimes you will find that the coma persists, notwithstanding all the support you can give; and then you may generally conclude with certainty, that the blood has become poisoned by pus, or some other morbid agent, and that death from pyæmia is about to occur, or that local formations of pus are about to be developed in various parts of the body.

"In those cases in which the disease responds to the stimulating treatment, the delirium subsides, and speedily altogether disappears; the redness and swelling diminish; the pulse becomes softer, fuller, and less frequent; the fever decreases, and the state of convalescence is rapidly established.

"Sometimes, through feeble powers of digestion in the patient, or injudicious zeal on the part of the attendants, you may find that you are over-stimulating. What are the indications of this? They show themselves in sickness, in flatulence, in a sense of oppression, perhaps also in derangement of bowels. When such symptoms occur, nothing can be easier than to suspend the treatment for a few hours, to give only a little cold water, and afterwards to resume it cautiously in diminished quantities.

"The upshot, then, of all I have to tell with respect to the treatment of erysipelas, is to give stimulants and nourishing food freely, and from the very commencement of the attack. Don't trouble yourselves with too much attention to the secretions, as some are apt to do, who imagine that the alteration of these by grey powder, black draught, *et hoc genus omne*, is necessary to the favorable issue of the case, but who, by the time they have got the secretions into what they conceive to be a correct condition, find that their patient is fairly slipping through their fingers, and is dying, worn out, and exhausted. As soon as you are satisfied that the patient to whom you are called is labouring under erysipelas, at once begin to administer stimulants and nourishing food, using the precautions I have mentioned; and what I wish above all things to impress upon you is, that this stimulating treatment should be employed from the very beginning of the

attack. With respect to the bowels, you must be guided by circumstances; if they are confined, you may open them by an enema, or by a dose of castor oil, or some other medicine, which will neither irritate the mucous membrane of the alimentary canal, nor exhaust the patient's strength; always keeping in view that the poison of erysipelas is exceedingly depressing in its action, and that the object of all your treatment should be, first, to antagonise the poison, and, secondly, to uphold the patient's powers so as to enable him to bear up against one of the most lowering and debilitating diseases to which the human frame is liable.

"Now of all the stimulants, I believe, as I have already said, the alcoholic are the best, and I have witnessed such remarkable effects in such a variety of cases produced by their free exhibition that I am inclined to consider them as *antidotes* to the erysipelatous poison; and if I were to be restricted to any one remedy in the treatment of this disease, I should, assuredly, choose brandy. With a commissariat well supplied with brandy, and simple means to keep the bowels open, I think I could engage to keep erysipelas at a minimum among the wounded in our army in the Crimea.

"Some attach great importance to the use of the tincture of sesquichloride of iron in this disease. I have no doubt many cases such as those which I have placed in my first group will get well under that drug, partly and mainly because it excludes depressing treatment, partly, perhaps, from some tonic power in the medicine; but I would as soon think of trusting to it in the treatment of the third or fourth group of cases, as I would to the billionth of a grain of aconite, or of arnica, or sulphur, or any other homœopathic absurdity. The remedy, so far as I know, is unobjectionable in itself, but its power to do good is small; and if you try it, let me advise you not to trust to it alone, but merely to use it as an adjunct to the treatment which I have endeavoured to impress upon you to-day. For, as I before said, there is a large class of cases of erysipelas which will get well without any treatment whatever, and, indeed, in spite of depressing treatment, because either the dose of the poison which these patients have imbibed has been very small, or else because their powers of resisting and holding up against the disease are very great; and in such cases you may amuse yourselves, if you like, with giving such remedies as the sesquichloride of iron. But in all severe examples of the malady, place your trust in food and brandy, freely given under careful regulation, and adopted from the very commencement of the attack."

ART. 13.—*Scarlet Fever occurring twice in the same person.*

By Dr. JOHN WEBSTER, F.R.S.

(*Edin. Medical Journal*, Sept., 1855.)

Well authenticated cases of this kind are very rare indeed, and hence the great value of a case such as the following, where any doubt as to the fact is altogether out of the question. Dr. Webster writes:

"Having recently attended an unequivocal example of scarlet fever affecting the same young lady at two separate periods, during both of



which attacks the patient was under my immediate observation, I am induced to relate briefly the chief pathognomonic features observed during both instances. And as at the same time several relatives witnessed the phenomena exhibited by the disease, on each occasion, throughout its entire progress, not the slightest doubt can be entertained respecting the distinct nature of the malady, which passed regularly through every phase. Indeed, some symptoms during the recurrent attack, proved exceedingly severe, and at one time were so marked, that considerable anxiety was felt respecting the result, which, however, soon became dissipated by the patient's favorable progress and ultimate recovery.

The following are the chief facts of the case observed at both periods: During the month of April last year, a nurse in the family of my patient had scarlet fever, then prevalent in the neighbourhood. All the characteristic symptoms of that eruptive complaint manifested themselves in this patient, who, in due time, recovered. Subsequently the eldest daughter, Miss M——, then in her nineteenth year, was also attacked by the same malady. Afterwards a second female servant, as likewise a younger brother and sister of Miss M——'s, thus making five distinct cases of the above epidemic disease, which occurred consecutively amongst members of one establishment, and all dwelling in the same house. Being perfectly cognizant of every fact specified, I would request particular attention to these important collateral circumstances, since they supply additional confirmation on the special point discussed.

The first symptoms in Miss M——'s primary attack of scarlet fever, which commenced on the 8th of May, 1854, were those commonly observed during ordinary invasions of that disease. The skin felt very hot, the pulse became greatly accelerated, the eruption appearing most copious over the chest, neck, and upper extremities. The febrile excitement now increased considerably; the throat was also affected, but not severely; the tongue soon assumed the strawberry appearance so common and pathognomonic in this malady, being also very red, swollen, and at first loaded with fur, but subsequently it got much cleaner. The eruption continued apparent during three consecutive days, then subsided, and after the fourth day the rash had entirely vanished. Subsequently every symptom of illness soon ceased; and the disease having passed through each stage satisfactorily, my patient in about a fortnight became convalescent.

Towards the end of March in the current year, scarlet fever again broke out in the same family, when a younger brother of Miss M——'s, but one who had escaped infection during the previous epidemic, now became attacked, every symptom being distinctly pathognomonic. The disease passed through its usual course, and constituted altogether one of the most severe examples of scarlet fever, ending favorably, which I have for some time attended.

Entertaining no fear of again becoming infected, and believing precautions wholly unnecessary against this reappearance of scarlet fever in the same house where it last year prevailed, Miss M—— assiduously attended her sick brother during the continuance of his malady. Indubitably this sisterly devotion mainly contributed towards producing my patient's second attack of that eruptive disease, of which the premonitory indications appeared on the 5th of April last, or almost eleven months after the primary occurrence of the identical malady in the same individual.

Towards evening of the day now mentioned, Miss M——, after feeling



languid and faint, was seized with pains in different parts of her body, succeeded by rigors; then headache and hot skin, accompanied with sore throat, which were followed by an eruption on the skin. However, it was not till next morning, or the 6th, when every doubt respecting the patient's actual disease was dissipated. On the 6th, the surface felt exceedingly hot, pulse very frequent, and the eruption now appeared unusually copious over the entire person, particularly on the neck and extremities; face became swollen, had severe headache, with delirium; also sore throat, and enlarged tonsils; the tongue red, and exhibiting quite a strawberry appearance. In short, all the usual indications of scarlet fever were now manifested, and highly characteristic of that specific disease.

During the 7th and 8th of April the malady proceeded in the customary manner, the febrile symptoms being, moreover, unusually severe. Pulse 120, with much delirium, as also considerable nausea and vomiting. The eruption continued exceedingly copious over the whole body; face a good deal swollen, red tongue, and loaded with fur on the centre, but exhibiting still the strawberry aspect on its tip and edges. Throat much affected; tonsils and uvula being greatly enlarged, so as to impede deglutition, even of liquids, and also to render breathing often difficult. Cough now supervened; there prevailed great restlessness, and the nights were sleepless.

On the 9th the eruption began to decline, the fever was somewhat ameliorated, the tongue got perfectly clean, but deep red, and swelled. Throat continued bad, tonsils being still much enlarged, and partially ulcerated, although deglutition, as also the breathing, were now somewhat improved. Pulse less frequent, and scarcely any delirium. Throughout this, and three previous days, the usual pathognomonic symptoms of scarlet fever were unequivocal, the present attack having proved much more severe than the former similar affection.

By the 10th the eruption had almost vanished. Pulse tranquil; skin cool and moist; bowels freely opened; urine copious; throat greatly improved, a small abscess having broken during the previous night; slept several hours consecutively; cough less frequent; breathing, and also swallowing, considerably ameliorated, and felt altogether much better. Very little fever; and, although languid, the patient showed evident indications of future convalescence.

Passed a good night on the 11th, the acute symptoms having almost ceased. Tongue clean; tonsils less swollen, and is able to swallow with more facility; bowels open; kidneys secrete copiously; but the patient still continued weak.

During the 12th and 13th the symptoms were ameliorated in every respect; appetite not so bad; free from cough; throat better; enjoyed several hours' sleep during the part of two nights, and is much improved.

On the 14th Miss M—— was able to get out of bed, being considerably stronger. Every day afterwards was marked by progressive improvement; the skin desquamated satisfactorily; and in twenty-one days from the first appearance of any eruptions I discontinued attendance, my patient having entirely recovered.

ART. 14.—*On Varioloid and Varicella.* By Professor TROUSSEAU.

(*Medical Times and Gazette*, Sept. 1, 1855.)

Many practitioners of high scientific repute believe that the same relationship prevails between varicella and varioloid as between this

last and variola. This it is impossible to admit. If we bring an individual having genuine vaccine scars in contact with a smallpox patient, he may take a varioloid, and, while suffering from this, he may communicate a true variola to a subject who has neither been vaccinated nor had the smallpox. If we take the pus from a varioloid patient, and inoculate a healthy person, as has been done in epidemics when vaccine lymph has run short, we produce the legitimate smallpox. These are so many proofs of the identity of the two affections. It is not thus with varicella. It neither arises from contact with varioloid, or is capable of communicating true variola. We see it arise just as easily in persons who have had that disease, as in those who have been exempt from it; in the unvaccinated, and in those who have been vaccinated. M. Trousseau has seen an epidemic of varicella at the Necker Hospital, which attacked all the children, a short time after vaccination had been quite successful. This is an important question in hygiene, inasmuch as varicella, of itself, is an affection destitute of danger; and we may leave the subject of it in communication with surrounding persons, without the fear of finding a serious malady developed. The same practice, pursued in varioloid, might give rise to a mischievous development of variola.

*Varioloid.*—Thirty-five years ago, an authentic example of smallpox after vaccination was unknown, although Jenner had seen examples of this, and had indicated them; but, as there are always to be found persons more royal than the king, so there were practitioners who accorded to vaccine more than he who had discovered and propagated it had claimed for it. In 1825, a very violent epidemic of smallpox prevailed in Paris, during which individuals who had been vaccinated were attacked. M. Husson, who was one of the Vaccine Committee in 1800, and one of the most ardent promoters of vaccination, contested the validity of these cases; and so extraordinary was the circumstance thought to be, that whenever a varioloid patient arrived at the hospital, the bells were loudly rung, in order to call as great a number of practitioners together as possible for the verification of the fact. An epidemic at Edinburgh, and two at Marseilles, multiplied examples. The attention of governments became aroused, and especially in Germany, where re-vaccination has been rendered obligatory. At the present day, there is no hospital in which we may not see persons having the vaccine scars the subject of variola, and even dying of it. It may occur even as early as the second or third year after vaccination; and M. Trousseau has seen an infant at the Necker Hospital take a genuine variola six weeks after a successful vaccination. A mother and her three children also took it soon after vaccination, and in the woman, who died, it was confluent.

At its onset varioloid differs in nowise from variola. Fever arises and continues until the eruption appears. We see, however, more frequently supervene a scarlatiniform or petechial eruption, but it does not influence the prognosis unfavorably, as in variola. The eruption does not differ from that of variola, until the eighth day; but at the eighth day from the commencement, or the fourth from the eruption, in place of tumefaction and inflamed aureola supervening, we find the integuments become pale and flaccid. The

pustules do not become larger, remain acuminate, and umbilicate but little. They dry without bursting, become rugous, and pass into the "horny" condition. Those of the limbs, in place of acquiring a size three or four times as large as those of the face, do not increase, and cornify in the same manner. By the tenth day the eruption is dry. In more serious forms, when the varioloid, as sometimes happens, is confluent, there is sometimes secondary fever; but at the tenth day, the tumefaction stops short, without any accident supervening, while in variola its doing so would be of fatal augury. The whole terminates with a rapid desquamation, although marks many remain, especially in persons with delicate skins.

*Varicella.*—When a child is brought to the Necker with varicella, the date of its admission is noted, and sixteen or seventeen days later, other children always exhibit the disease. If, on the contrary, it had been a smallpox case, other cases would have been observed from nine to eleven days afterwards—showing that the period of incubation is very different in the two affections. A child, in good health, whether vaccinated or not, whether having had variola or not, becomes suddenly the subject of a sharp attack of fever, there being present neither vomiting nor lumbar pain. The next day, or sometimes even the same day, fifteen or twenty red points are observed upon the skin, and some hours later the epidermis is raised. Twenty-four hours after the appearance of the red points, we observe bullæ or phlyctænæ, quite rounded in form, and transparent, as if they contained water. They resemble sudamina, magnified from ten to fifteen times. In variola and varioloid the eruption never assumes this bullar form. In those diseases, too, the fever and the eruption continue until the latter is completed. In varicella the phenomena take place successively. There is a day of complete apyrexia, the fever comes on during the night, and next day we find from thirty to forty points of eruption. The same takes place during the next twenty-four hours, and so on for four or five days, so that we have four or five successive eruptions. Twelve hours after the appearance of the eruption there is a limpid bulla formed, and forty-eight hours after the liquid has become lactescent, which is never observed in any form of variola. In variola discreta the eruption is of a very regular, rounded form, like a drop of wax dried upon the skin; but after two or three days the bullæ of varicella become unequal, irregular, and puckered, but never offer any appearance of umbilication. When pus begins to form in the phlyctænæ, a livid red, inflammatory aureola is produced, larger in size than the variolous aureola. When the pustule bursts it leaves a dark brown scab, having nothing in common in appearance with the yellow scab in variola, but much resembling that of echthyma. From twelve to fifteen days are required for the complete evolution of a variolous pustule, while four, or at most five, days suffice in varicella. So little dangerous is this affection, that M. Trousseau knows of no example of its having terminated fatally. Still, in some children, who manifest the purulent diathesis, it is followed by successive eruptions of pemphigus, which terminate by exhausting the patient and causing death. But these deaths cannot be imputed to the varicella itself.

Thus, then, variola and varioloid are identical; while varicella is



distinguished from these by the differences in its period of incubation and febrile paroxysms, by the form and duration of its eruption, and by the absence of danger.

ART. 15.—*On the prevention of pitting in Smallpox.*

By Dr. JAMES WALLACE.

(*Glasgow Medical Journal*, July, 1855.)

After trying several means for preventing pitting in smallpox, Dr. Wallace arrives at the conclusion that gutta-percha dissolved in chloroform (as originally suggested by Dr. Stokes) is more effectual than any other; but that even this will not entirely prevent the deformity. He finds, also, that it is sufficient to make the application just before maturation is complete.

“In 1849, when acting as clinical clerk in the Glasgow Royal Infirmary, I painted with collodion—as originally suggested by Dr. Ranking, in the ‘*Lancet*’ for January of that year—one half of the face of a patient labouring under semi-confluent smallpox, and was so much struck with its efficacy in mitigating disfigurement, that, on taking charge of the Grenock Infirmary, I determined on giving it a farther trial. I employed it, accordingly, in eleven cases, coming under my care in the spring and winter of 1852. Of these, five were confluent, two semi-confluent, three with the eruption discrete and moderately copious, and one with the eruption discrete, but very abundant. In one, the collodion was applied on the first day of the eruption; in another, on the second; in five, on the third; and in four, on the fourth. In all of them it was used some days previous to the stage of maturation, and the result in general was satisfactory, the pitting having been prevented to a considerable extent. But during the progress of the disease, the application was attended with no small inconvenience; for, when tumefaction set in, the collodion, from its want of elasticity, kept the parts in such a state of pain, heat, and tension, that several of the patients were with difficulty persuaded to let it remain on. For the same reason, also, it cracked so frequently, that gaps, which were formed here and there, had to be filled up by a reapplication of the solution, which, on subsidence of the swelling, became still farther necessary, because of the less prominent parts of the face being separated from the artificial pellicle. Nor were these disadvantages compensated for by anything like what has been called *abortion* of the pustules, for the progress of the eruption was just as if the face had been without any protection whatever, the only beneficial result having been mitigation of the pitting. Now, as this was the principal object in view, it became evident that the same effect was as likely to be produced, and with much less uneasiness to the patient, by having recourse to the application at a later stage of the disease; and as the efficacy of such a covering could depend, from what was observed, only on the exclusion of atmospheric air from the pustules when fully developed, and on its allowing cicatrization to advance in a way analogous to what is termed the modelling process,\*

■ Miller’s Principles of Surgery, p. 199.



the most suitable period seemed to be that immediately before complete maturation, when there is usually found a greater or less amount of swelling. By this method the face, on any subsequent increase in size, would be subject to considerably less constriction than by that first followed; and, on falling to its normal dimensions, would have the coating adherent to a much greater extent. But there would still be a great drawback in the inelasticity of the collodion, as well as in its liability to crack. I therefore gave up the use of that substance altogether, and had recourse to a solution of gutta percha in chloroform,\* which I have now employed in twenty cases,† four being confluent, seven semi-confluent, seven with the eruption discrete and moderate, and two with the eruption discrete but copious. In the whole of them it was painted on *immediately before complete maturation*, which occurred in three on the fifth day of the eruption, in three on the sixth, in ten on the seventh, in two on the eighth, in one on the ninth, and in one on the tenth. Under this plan, the increase that took place in the swelling was found to be in general moderate, and not such as to prevent the gutta percha yielding readily to it, nor to cause that feeling of heat and tension which was so much complained of by the patients subjected to the collodion application; and when the tumefaction subsided, the mask was found to adhere intimately to all the subjacent parts, except at the angles of the mouth, where, from the frequent motion of the lips, it invariably became detached. But this was easily remedied, either by touching lightly with a camel-hair pencil dipped in chloroform, the loose portion of the covering, which in that way became softened and collapsed upon the skin; or, which is better, by painting anew the exposed part of the face, without disturbing the separated pellicle at all. Farther than this no interference was necessary, the whole coating having been allowed to remain on till desquamation spontaneously occurred, when the face was found, in all the cases, to be in a very satisfactory condition. The ultimate result, too, was exceedingly favorable, scarcely any pitting having been noticed when the patients were dismissed, except in two or three instances, in which, however, it was moderate, and in which, but for the covering, there would, I am convinced, have been frightful disfiguration. At the same time it ought to be stated, that those cases which I had an opportunity of seeing, some months after their discharge, presented the marks much more distinctly than when under observation in the hospital, but still so modified, as to show that they had been very materially benefited by the means resorted to.

From this it will be seen, that the gutta percha, like the collodion, though to a much greater degree, succeeded only in mitigating, and not entirely preventing, pitting. Hence, some may be apt to conclude that its efficacy was much inferior to that of other applications, which

\* This seems to have been first used in cases of smallpox by Dr. Stokes of Dublin. Vide paper in 'Dublin Quarterly Journal of Medical Science,' for August, 1852, by the late Dr. Graves. According to the latter observer, it should not be employed till the eruption is *fully matured*, or even begins to exhibit the first appearance of collapse; but this stage is highly objectionable, in consequence of the cuticular covering of the pustules being then so much thinned, as to break readily while the solution is being applied.

† The whole of these, as well as those treated with collodion, are exclusive of fatal cases.

have of late years been brought under the notice of the profession. But in estimating the comparative merits of these, regard should be had to the appearance of the patients, as exhibited *long after* they had been under treatment, as well as on their leaving the hands of the physician."

### (C) CHRONIC DISEASES.

#### ART. 16.—*On Quinoidine in the treatment of Intermittent Fever.*

By Dr. DA COSTA.

(*Philadelphia Medical Examiner*, May, 1855.)

In this paper Dr. Da Costa gives a tabular abstract of fifty-three cases of intermittent fever which were treated with this drug. The patients, for the most part, were Irish of the lower classes, who applied for advice at the Moyamensing House of Industry, and the type of disease was, almost without exception, quotidian. The average dose required to arrest the disease in an adult was twenty grains, six grains being given shortly before the expected paroxysm, and the rest during the interval. Of the fifty-three cases cited (in many of which the disease is stated as being of long standing), the chills were arrested in forty-nine cases by the first twenty grains. Only four required a repetition of this quantity. In ten cases the disease returned, but this is not to be wondered at, when it is considered that a minimum of medicine was given to the patients.

#### ART. 17.—*Intermittent Fever treated by Olive Leaves.*

By Mr. SPENCER WELLS.

(*Medical Times and Gazette*, Aug. 11, 1855.)

These cases were treated in the Civil Hospital at Smyrna. The medicine was given in the form of a decoction made by boiling two ounces of fresh leaves in a pint of water.

CASE 1.—J. H., 62d Regiment, æt. 34, had intermittent fever in India in 1845. He was in hospital twenty-one days. The first eight or nine days the attack recurred daily; afterwards every other day. He remained free until May, 1854, when he had a slight attack in Malta. The next was in February last, in the Crimea. He had another in April, and was in hospital twelve days, the attack recurring daily. He had two attacks on board the vessel which brought him here, on the 4th of May. No treatment was adopted until the 9th, when he was ordered 3 grains of quinine three times a day. On the 11th he had the first attack in hospital, and was ordered 2 oz. of the infusion of olive leaves three times a day, the quinine being discontinued. The infusion was continued until the 17th, when he had another attack. The infusion was then changed for a strong decoction, which he continued until the 24th. He has had no return of the paroxysm since commencing the use of the decoction—upwards of six weeks ago. He has been kept in hospital since, in order to dilate an old stricture in the pendulous portion of the urethra.

CASE 2.—J. H., 41st Regiment, æt. 25, never had intermittent fever until he was at Varna, in July, 1854. He was then in hospital two months with quotidian. After he went to the camp, attacks occurred irregularly; he

would be in hospital for a few days, and then go to duty. They occurred regularly every other day for some days before he left, and he had one attack on board ship. He arrived here on the 4th of May, but had no attack until the 17th. On the 18th, he was ordered a strong decoction of olive leaves every four hours, which he continued regularly until the 28th. He had one attack after commencing to take the medicine, but has since been quite free. He remains in hospital on account of granular conjunctiva.

CASE 3.—F. D., æt. 26, Chatham orderly, had intermittent fever in Poonah and Hyderabad in 1852. It recurred at first every third day, afterwards every ninth day, for seven or eight months. He arrived in England in July, 1854. Had no attack in England. After arriving here in the spring he had two very slight attacks, but did not apply for medicine. On the 18th of June he had a smart attack at 7 p.m., which recurred at 11 a.m. on the following day. He took an emetic and effervescing draughts, and on the 20th the decoction of olive leaves was ordered every four hours as before. This he continued until the 26th, when he complained of pain over the liver, and the stools became light-coloured. Half a drachm of the acetate of potass was given three times a day, and diluents used freely until the 1st of July; and he was discharged to duty on the 3d.

#### ART. 18.—*Researches on Gout.*

By Dr. W. BUDD, Physician to the Bristol Royal Infirmary.

(*Lancet*, June 23, 1855.)

In a communication to the Royal Medical and Chirurgical Society Dr. Budd tells us that, in 1852, he had the opportunity of examining portions of the cartilages of the knees and elbows, and the adjacent bursæ, from a man who died after having long suffered from gout. Subsequently, he has examined the knees, elbows, wrists, ankles, and most of the smaller joints of seven other gouty subjects, and has found them to contain a deposit consisting of lithate of soda in combination with phosphate and carbonate of lime. The nature of the deposit was very different in the cartilages and bursæ. In the former it consisted almost solely of lithate of soda; in the latter there was an admixture of the calcareous salts, sometimes in large proportion. He found that the deposit in the cartilages was not placed on the free surface, but that it formed a thin layer immediately beneath the surface in the substance of the cartilage itself, this layer being also, in early stages of gout, often separated from the edges of the cartilage by a free margin, in which no deposit existed. He traced the deposition of the foreign matter as occurring, in many instances, around the cartilage cells. The lithate of soda, he, in common with many others, supposed to be an essential ingredient of the *materies morbi* of gout, but the calcareous salts to be only an accidental complication, originating in the common inflammation around the gouty joints. He then details the particulars of two cases of acute gout, in one of which he found a considerable quantity of urea (a known product of the metamorphosis of lithic acid) in the serum, obtained from a blister, and in the other from the blood; and he adds, that in nine other cases he had procured a large quantity of urea from blood or serum obtained from the patients, there being no albuminuria, and the kidneys being, in all, as far as could be



ascertained, free from disease. From the facts and reasonings founded on them Dr. Budd is led to the following inferences: 1st. That lithic acid (in combination with soda) is an essential and very principal ingredient of the *materies morbi* of gout. 2. That the result of the fit of gout is partly to deposit this compound in the least vascular structures of the joint, but much more largely to accomplish its elimination from the body, by converting it into compounds of a more simple order, such as urea, and admitting of more ready discharge from the body. (The paper is accompanied by drawings and microscopic preparations.)

ART. 19.—*Cases of Leucocythæmia.* By Dr. WILKS.

(*Guy's Hospital Reports*, 1855).

Dr. Wilks relates two of these cases, the one certainly, and the other probably, connected with disease of the spleen. In another case, and only one—that of a man dying of typhus—were the white corpuscles formed in any considerable excess; and yet upwards of fifty cases of a dyscratic character, such as anæmia, scurvy, and ague, were examined with a view to the settlement of the question, whether the excess of these corpuscles is really connected with disorder of the spleen. The cases are as follows:

CASE 1.—Sarah R—, æt. 40, was a married woman, living at Chatham, where she had resided all her life. She had ague when a child, but not since. About two years before her admission into the hospital she observed a swelling on the left side of the abdomen, and this had been gradually enlarging since that time. It had given her no pain, and only inconvenienced her from its size. She had, however, become thinner and paler, and her health had altogether suffered much. On admission, under Dr. Babington, on April 20th, 1853, she was seen to be a pale, thin, cachectic looking woman, and so weakly that she was obliged to keep her bed the greater part of the day. She was also short-breathed; had palpitation of the heart; and there existed a soft systolic bruit over the valves. The urine was albuminous. The abdomen was enlarged, from the presence of a tumour extending from under the left hypochondriac region nearly to the pubes; it had a flattened surface, and its inner edge occupied the mesian line, and thus clearly, from its position, it was an enlarged spleen. I pricked the arm of the patient, in order to examine the blood, and found the red and white corpuscles in nearly equal proportions. In some parts of the specimen, which was contained on a plate of glass, the white corpuscles exhibited a superiority of numbers.

The patient remained in much the same state until the 2d of May, which was her menstrual period, when a severe hemorrhage occurred from the nose. Previous to this time, she stated that the catamenia had been regular. The bleeding having continued for some hours, and the patient becoming very exhausted, plugging was had recourse to. The menses did not appear. Great anæmia was left in consequence of this attack, and the recovery from it was hardly accomplished, when, on June 2, the hemorrhage again returned from the nose, and plugging was again found necessary. The blood, as well as that obtained from the arm, was in the same condition as when first observed, and that which fell into a vessel coagulated as usual.

On July 4, and August 6, the hemorrhage again occurred, as at the com-

mencement of the previous months, and this consequently produced an increasing weakness and anæmia of the patient. At the commencement of September, she entirely kept her bed, and was in a much more debilitated condition than when she first entered the hospital. The bleeding, which now recurred, was much less than on previous occasions. All other symptoms were the same, including the large amount of colourless corpuscles in the blood, which amounted sometimes to an excess over the red,—the tumour in the abdomen, and the albuminous state of the urine. She was now exceedingly anæmiated and low, but had no dropsy.

During the month of September she daily became weaker, and, towards its close, she was evidently fast sinking from exhaustion, and, on October the 1st, she died; but without the occurrence, this time, of any hemorrhage.

*Inspection, nineteen hours after death.*—The body was thin and pale; there was no anasarca of any part, but a few purple patches existed on the extremities. The lungs, with the exception of a little chalky deposit at one apex, were healthy. The heart was small, pale and flabby. Liver healthy, and gall-bladder full. Pancreas healthy. Kidneys granular, and containing many cysts. The large intestines very much diseased, from old and recent ulceration, and hypertrophy of muscular coats. Cysts in ovaries. The spleen was immensely enlarged, and identical with the tumour in the abdomen observed during life. It was long and narrow, and reached nearly to the inguinal region. My note says—Its weight was  $2\frac{1}{2}$  lb, but I think it must have been much heavier than this. A section presented nothing remarkable in its appearance, except being more firm and dense than commonly seen, and which made it approach in character to that of liver. The microscope discovered in it nothing more than the usual splenic elements, and thus the enlargement seemed due to a simple hypertrophy of the organ. The blood from all the tissues was everywhere alike, having a large excess of white corpuscles—that of the mesenteric vein being of the same character as the blood from other parts.

CASE 2,—Edward T—, æt. 40, was a shoemaker living in the Borough. He had resided in the neighbourhood for many years, was never in a miasmatic country, and never had ague. His habits were temperate. He had been ailing for about nine months before his admission, but complained of nothing in particular, and for three weeks only had noticed an enlargement of the abdomen. When first seen he was rather spare and sallow, but otherwise in apparent good health, and complained only of debility and a slight cough. On the left side of the abdomen was a hard tumour, smooth on the surface, passing from the left hypochondriac region as far as the anterior spinous process of the ilium, and at its inner edge, reaching to the median line. His arm was pricked, and the blood examined microscopically, when the red and white corpuscles were seen to exist in nearly equal proportions. Subsequent observations exhibited always the same appearances. The man continued in the hospital for two months in much the same condition, having used iodine ointment locally, and every variety of medicine internally, but without any effect in diminishing the size of the tumour. At the end of this period he left, in the same state as on his admission, being not very ill, making no particular complaints, and being able to walk about all day. He has not been heard of since.

ART. 20.—*On Beri-beri.*

By Dr. MOREHEAD, Professor of Medicine in Grant Medical College.

(*Transactions of the Medical Society of Bombay*, 1855.)

*Beri-beri* is a term applied to a train of dropsical symptoms, originating, according to Dr. Morehead, in the scorbutic diathesis, under some unusual exposure to cold. There is nothing special in the affection, and therefore no good, and some harm, arises from retaining the special name. This view of the pathology of the disorder, in Dr. Morehead's opinion, is well borne out by the particulars of the development of the disease in a ship's crew, which recently came into the harbour of Bombay, as well as by the state of some of the men who were transferred from the ship to the Jamsetjee Jejeebhoy Hospital, and we quite agree in this opinion. We extract the evidence of the captain of the ship given before the coroner, premising a sketch of the disease in its ordinary forms. We have not space for the cases, which, however, contain nothing remarkable :

"The symptoms of this disease sometimes advance gradually ; at other times they develop themselves suddenly. When they have been gradual in their approach, the individual for several days experiences a sense of weakness, and inability or unwillingness to exert himself, and shortly afterwards to these symptoms are added pain, numbness, stiffness, with more or less œdema of the lower extremities. There is also more or less dyspnœa experienced, with a sense of oppression and weight at the epigastrium. The œdema does not continue confined to the extremities, but extends to the trunk and face, occasioning a general puffed and bloated appearance. The weakness of the limbs and the dyspnœa are particularly complained of on motion. As the disease advances, the dyspnœa increases, the face becomes more swollen and bloated, and the lips livid. The numbness of the limbs increases to such an extent that they become almost paralytic, the oppression at the epigastrium becomes aggravated, and frequent vomiting is excited, and the ejected matters are sometimes mixed with blood. The urine is scanty and high-coloured, sometimes almost suppressed ; the thirst is great ; the pulse is at first quick and small, or unaffected, then it becomes irregular, intermittent, and fluttering. Palpitations are experienced, attended with a sense of suffocation, a sinking pulse, and death.

"These trains of symptoms may run their course in from two to three weeks ; or the course may be much more rapid, and, when so, the numbness, the stiffness, and œdema of the lower extremities, become quickly followed by the dyspnœa, the palpitation, and the sinking pulse.

"Now, what are all these but the symptoms which attend more or less on serous effusion into the connecting areolar tissue of the extremities, the cavity of the abdomen, that of the pleura, the pericardium, or into the air-cells of the lungs, and their connecting areolar tissue—in fact, the symptoms of general dropsy more or less extensive, more or less quickly forming. Dr. Watson, in his excellent lectures,



thus writes of dropsy :—‘ Now, from whatever cause this watery condition of the whole body may arise, the effects resulting from the presence of the *water* are the same : and of what do patients in this state usually complain ? Why, of shortness of breath and palpitation of the heart ; of a sense of impending suffocation if they attempt to lie down or actively to bestir themselves, of tightness and distress across the epigastrium, relieved somewhat by eructation, augmented by food and drink ; of weight and stiffness of the limbs, and sometimes of drowsiness.’

“ Now let us inquire what, in fatal cases of beri-beri, are the morbid appearances found after death. Why, dropsical effusions into the subcutaneous areolar tissue, œdema of the lungs, effusion into the sac of the pleura, into the pericardium, into the peritoneal cavity, and into the cranium. In some cases traces of old or recent inflammatory action of internal viscera may be found ; but these form no essential part of the disease.”

The evidence of the captain, William Eames, is as follows :—

“ I am master of the ship *Faize Allum*, of the port of Bombay, and have been constantly commanding, or been chief officer of vessels trading out of Bombay, with a lascar crew, since the year 1838. I last left Bombay on the 3d day of June, 1852, with a lascar crew of sixty-five men and boys ; and the deceased, Bhana Moorar, aged about forty years, and deceased Jadow Dewa, aged about twenty-five years, both Hindoos, formed part of the crew. We proceeded from Bombay to Singapore, and from thence to Siam, and returned from thence to Singapore, and so back again to Siam ; and from thence to Singapore, which place I quitted for Bombay on the 3d March this year, expecting to make the voyage in seven weeks, the average passage being about two months. I had on board curry-stuff, rice, water, dall, ghee, salt, &c., as prescribed by the regulations, with a good supply of water ; and during such time as the ship was in harbour always supplied the crew with greens, fresh fish, and fresh provisions. The crew all remained healthy till about the 21st day of May last, in latitude 10° N., longitude 64° W. We had then been two months and eighteen days at sea. On the 15th day of April I was within about seventy miles, or thereabouts, of the Island of Ceylon, but being unable to stand the strong current and west winds then blowing, after consulting with my serang and chief officer and passengers, I determined on relinquishing the attempt to get round Ceylon, and bore away for the Line, to come up to Bombay by the southern passage, round the Laccadives and Changos, and ran to the south of the Line as far as 8° 49', and then to the westward as far as 63° W., and crossed the Line again, running north, about the 6th or 7th May, and during most of the time had rain and squalls. Most of the water having been consumed, we filled up the water-casks with rain-water, collected on the surface of a clean awning. After making the Line, on the 6th of May, we had light weather, with occasional squalls and constant rain, and came on with the south-west monsoon up to 16° N. latitude on or about the 2d June, and arrived in the harbour of Bombay on the 6th June. I consider that I first fell in with the south-west monsoon about three degrees north of the Line. The

crew were all healthy up to the 21st of May. When in latitude  $10^{\circ}$  N., longitude  $64^{\circ}$  W. symptoms of disease first showed themselves. The deceased, Jadow Dewa, complained of pains in his feet, and loss of strength down the legs, and pain in the chest, with difficulty of breathing, and constipated bowels. I gave him jalap and cream of tartar, and, to rub on the chest, hartshorn, laudanum, and sweet oil. The crew, since the 15th of April, had been on a reduced allowance of about ten pounds in ninety pounds of rice—fish and water full allowance, the latter being rain water. Between the 21st day of May and 6th of June, eight other men were seized in the same manner, and all died; the average suffering about four or five days; a Portuguese sepoy died in three days. The deceased, Jadow Dewa, appeared to be recovering fast, and left the ship on the evening of the 6th of June. Bhana Moorar also appeared convalescent, and left the ship in my dingy. All the survivors of the crew are landed, the voyage being completed. The passengers, twelve in number, natives, and myself and officer, and the majority of the crew, are well. We drank the rain-water very freely, and I believe the deceased died of a disease called the *beri-beri* of Ceylon. I had a good medicine-chest on board, and treated those taken ill according to the instructions laid down in Dr. Thomas's book of medicine. We had no liquor on board the ship. I offered the crew pickles and vinegar, and also sugar, but they refused to eat it. The passengers and myself used pickles, sugar, and vinegar freely, but the crew declined till latterly. The whole number who were attacked were about thirty-five, of whom ten have died. We were in the latitude of Cochin when the disease first appeared, and were about  $10^{\circ}$  to the westward of the coast of India, with light north-west and north-east winds. The crew were protected from wet as far as possible. The disease attacked persons of all ages, but principally the old and more infirm of the crew. Further I know not. The cargo consisted principally of sugar in bags, of Malling ivory, teakwood, plant and sapan wood, and raw silk. The hatches were kept constantly open when the weather would permit, the fore-castle well cleansed and fumigated with powder burnt and benjamin.

"The jury returned the following verdict:—'Deceased died of *beri-beri*.'"

The cases transferred to the hospital were ordinary cases of the affection. They did not present unequivocal signs of scurvy, but there were decided scorbutic tendencies. There was not time for the full development of these signs in these feeble dropsical people. As to treatment, this must be conducted on general principles, like any other case of dropsy in a scorbutic diathesis.

ART. 21.—*On the removal of metal from the system by Galvanism.*  
By Dr. G. HUFF, of Lexington, Kentucky.

(*Philadelphia Medical Examiner*, Aug., 1855.)

Dr. Huff relates three cases in illustration of the possibility of employing galvanism as a means of removing metallic substances out of

the body, according to the plan proposed by MM. Poey and Vergnès ('Abstract,' vol. xxi. p. 288). This plan is very plausible, but it is questionable whether it will gain much weight from the cases which are now adduced in its favour.

CASE 1.—Mrs. W——, æt. 27, of lymphatic temperament, with auburn hair and white skin, had been under treatment for diseased spine fifteen months. During this time she had taken very large quantities of mercury, which (her subsequent medical attendant stated) produced paralysis of the lower extremities.

I was called in consultation by the advice of her physicians, and it was decided that she should be put under treatment by galvanism. Her physicians having thrown upon me the entire responsibility of the case, I took charge of her; and one day, while making an application of this potent agent to the spine, the feet having been placed in a metallic bathing-tub with acidulated water, her husband suddenly called my attention, exclaiming at the same time, "See the mercury!" On making an examination, I found several globules of metallic mercury lying on the bottom of the tub. I continued this (electric) treatment a long time, and she ultimately recovered, and now enjoys the powers of locomotion most perfectly.

CASE 2.—Mr. B——, æt. 40 years, of nervous temperament, with dark hair, white and thin skin, had been treated for syphilis for a long period, and had been repeatedly salivated, from which he had suffered severely in the joints. The capsular ligament was so much elongated as to cause luxation of the head of the *femur*; separation of the *carpus* of each hand had taken place, and the metacarpal joints of the fingers were very much enlarged. He had been under treatment of physicians who stood deservedly high in the profession, and had visited warm springs in Arkansas by their advice. At this time he could scarcely move with crutches, even with the help of two attendants which he took with him. He remained there one winter, and returned without having obtained any relief. His friends then advised him to apply to me, and, with the consent of his physicians, he did so. On examination of his case I concluded to treat him, and commenced with warm baths, which invariably left him worse, the joints becoming more stiff and painful, and with less mobility. Believing that the remote cause of this aggravation of his disease was the presence of mercury in his system, I was induced to attempt to extract it. To accomplish this, after having placed him in a porcelain bathing foot-tub, with acidulated water, and a metallic plate beneath his feet, I completed the circuit, and after the lapse of twenty minutes I discovered a light-white precipitate, and the impress of his toes left on the plate of a light bluish colour, with silvery lustre. I repeated this operation several times, and then commenced the galvanic treatment for rheumatism, and infused iodine into the joints, in order to produce absorption of abnormal secretion that had formed there. From this time he commenced to improve, and went on improving without a relapse. All the joints have now recovered their normal condition, with the exception of the left hip-joint, the femur of that side now remaining seven-eighths of an inch below the right, although it has ascended three-eighths of an inch during my treatment; his general health has very much improved; in fact, he says it is now as good as it has been at any period of his life.

CASE 3.—Mrs. N——, æt. 28 years, of bilious temperament, small size, hair and eyes black, of a very high order of intellect. At the birth of her second child there was very profuse hemorrhage, and much inflammation of the uterus was superadded in consequence of medicines having been impru-



dently given by her physicians to facilitate labour. For the purpose of suppressing the hemorrhage and restoring the uterus to a healthy condition, sugar of lead was given in small doses, and its use continued a long time. This treatment resulted in *lead palsy* (the total loss of muscular contraction of the extremities). In order to extract the lead from her system, I commenced the treatment by galvanism in the same manner as in the foregoing case, and with the same results, except that the precipitate was of a dark grey colour, and the impress of the toes left on the plate was of a darker hue. When the paralysis was nearly removed, partial amaurosis set in, and ultimately became total. I treated this without benefit, although I think the treatment has not been fully tested, as she was obliged to return home, in consequence of domestic cares, sooner than I anticipated.

ART. 22.—*Supposed case of Ergotism.*  
By Mr. THOMAS CAMPS, of Fenny Stratford.

(*Medico-Chir. Review*, July, 1855.)

There is every reason to believe that this was a true case of ergotism. There is, no doubt, some obscurity as to the cause, but there is every presumption that this was the eating of badly harvested corn—corn which had become diseased by lying long upon the ground before it was reaped; and this presumption is greatly increased by the fact that the autumn before the time when the disease declared itself, was unusually wet and stormy. This presumption, indeed, is wellnigh converted into a certainty by the collateral evidence, and particularly by the history of a similar disease which is recorded by Dr. Wollaston and the Rev. J. Bones, in the ‘*Philosophical Transactions*,’ for 1762. At any rate, there is no reason to believe that the patient had eaten any rye.

The case is related as follows, the notes showing the resemblance between the symptoms and those of actual ergotism.

CASE —“James Golding, of Newton Longville, Bucks, labourer, æt. 25 years, about five feet nine inches in height, well formed, with dark brown hair, grey eyes, and, when in health, of florid complexion, had been working in the neighbourhood of London since Michaelmas, 1853, and was employed in digging drains for some houses about to be erected. A little after Christmas he was attacked with pneumonia, which his medical attendant informs me was severe in character, affecting both lungs, and requiring general and topical bleeding, tartar emetic, and mercury, to subdue it. Whilst improving under this treatment, he imprudently exposed himself to cold. The urine now became albuminous, and the patient’s condition in all respects more unfavorable. Shortly after, he was removed by his friends to his native village and put under my care on the 26th of April, 1854.

“I found him reduced to a state of great debility, much emaciated, and complaining chiefly of difficult breathing, and a sense of exhaustion. The left lung appeared to be extensively hepatized posteriorly, much more so than the right, which also showed well-marked signs of disease. I was unable to detect any morbid sounds in the heart, nor were any observed by such of my medical friends as examined the case. The urine was albuminous, but by no means to a great extent, and the mouth suffering, though not severely, from the effects of the mercury. The pulse, as might be expected, was feeble.

No febrile action was present, and at that time there was nothing to lead me to anticipate the remarkable symptoms about to be developed. As no indication for active medication existed, it appeared the safest plan to watch the patient carefully, but in his present state to pursue only the expectant plan of treatment.

"In the course of ten days or a fortnight after his return home, he began to complain greatly of pain and numbness in the left leg and foot.\* A small patch of eruption showed itself on the calf of the leg (much resembling lichen in appearance, except that it was darker in colour), very slightly raised above the skin, and not mingled with any vesicles.† There was no heat of surface, though the patient complained of a sense of burning, accompanied with formication; and there was nothing in the appearance of the limb to account for the constant and intense pain, which continued day and night, without being relieved by any form of opiate, though given in large doses. In fact, so completely did opiates fail to mitigate the pain, that they were abandoned as useless. After a few days, the foot and lower part of the leg became cold, nearly void of feeling, and were evidently in a state of approaching gangrene, which soon showed itself unequivocally. The parts became black, and so shrivelled as to give the idea of nothing intervening between the skin and the bones beneath it. At this time, and throughout the greater part of his illness, his extreme aversion to warmth was very remarkable. He complained that heat applied to any part of the body aggravated his pains; and if, on a cold day, any additional covering was laid upon him whilst sleeping, he wakened almost instantly and threw it off.‡ I generally found him, in the coldest weather, lying in bed with only an old cloak thrown over him.

"When sphacelus had taken place, the affected limb became easier; but about this time the right leg and foot were affected in a precisely similar manner, and, in succession, both hands, the ala of the right nostril, and a small portion of the upper part of the helix of each ear. In the earlier part of these attacks he suffered greatly from spasmodic contraction of the hands and feet, which was not constantly present, though generally returning at short intervals.§ The pulse in frequency differed but little from the standard of health.|| There was no febrile action,¶ except at the time when vesications appeared where the line of demarcation was about to be formed. Though not complaining of the head, the mental powers were evidently much enfeebled. The memory was greatly impaired, and his attendants described him as talking all kinds of nonsense.\*\* As soon as the pains abated,

\* *Primum symptoma erat torpor crurum, tum dolor cum levi tumore sine inflammatione, et citâ successione, frigus, livor, sphacelus, membri excisio.* Tissot's letter: *Philosophical Transactions*, vol. xlii, part 2, 1762.

† *Maculæ in alterius pedibus effluere, pulicum morsibus similes, quæ in octavam hebdomadam perdurant.* Quorundam facies turpiter fœdata, est maculis. J. A. Sinc, quoted by Tissot, as above. See, also, a case quoted in the *Medical Times*, March 4, 1854.

‡ "*Dolore interno cruciabantur atrocissimo, qui ex atmosphære aut lecti calore enormiter augebatur.*" Langius, as quoted by Tissot.

§ "*Cette maladie,*" dit M. Bordot, "*commence le plus ordinairement par une sensation incommode aux pieds avec une sorte de titillation, ou de fourmillement dans ces parties. Ces symptomes sont bientôt suivis de contractions violentes spasmodiques des membres,*" &c.

|| "*Pulsus sanorum similis, nullo excepto.*" J. A. Sinc, as quoted by Tissot.

¶ Langius describes the disease as commencing, "*absque ullâ febre.*"

\*\* "*Obliviscuntur se . . . mentis minime compotes.*" J. A. Sinc.

he began to perspire profusely, slept well,\* and had a voracious appetite.† The bowels continued regular, with the exception of about a fortnight, when he had severe diarrhœa. When signs of approaching gangrene became evident, wine was given, and bark in its different forms, as long as the patient was willing to take it.

"The specific gravity of the urine, which for a considerable time was examined weekly, was 1·011 when he first came under my care, but after a short time it was reduced to 1·006, from which it afterwards varied but little. The colour was generally pale, and it was either neutral or faintly acid, depositing little or no sediment. The albumen continued to diminish, till eventually it almost disappeared. From its low specific gravity no sugar could have been anticipated, nor did it at any time show traces of it. At an *advanced* period of the disease, the presence of urea was clearly ascertained, though I am unable to say whether this element was normal in amount.

"The eruption, which constitutes an interesting feature of the disease, continued to make its appearance at intervals throughout the attack, till within a month of the present date, Nov. 12th, 1854. It was most abundant on the knees, shoulders, elbows, and the skin covering the lines of the tibia and ulna. The face had many spots on it: they were observed on the nose, the upper part of each ear, and even on the glans penis. Indeed, no part of the body appeared to be wholly exempt from them. The eruption was accompanied with intense itching. Its duration was uncertain; sometimes disappearing in a few days, and at others continuing for many weeks. The spots generally appeared in small patches, varying in form. They differed from petechiæ in colour, being of a redder tint, and were slightly elevated above the level of the skin. On dying away, they left in some places merely a dark stain; in others, desquamation of the cuticle took place, or incrustations of a yellow colour were formed, adhering for a lengthened period; whilst occasionally, as on the nose and ears, slight sloughing of the parts affected took place.

"No fetor was perceptible from the gangrenous portions of the limbs till their separation commenced. It was then horribly offensive. On this account, after the soft parts of the *left* leg had separated to such an extent as to leave a large portion of the bone exposed, it was thought best to remove the limb at this point. This was done by sawing through the bones close to the granulating surface, which was affected with difficulty, from their extreme density. The walls, where divided by the saw, presented a polished surface resembling ivory. It was found needful to apply lint to the bone, which bled freely. I was not allowed to retain the foot after its removal, but on separating a portion of the posterior tibial artery it was found, contrary to expectation, pervious. The bones of the *right* leg soon after became denuded a little above the ankle, and the foot appeared about to separate at its articulation with the leg, the ligaments being completely exposed. Amputation, in the ordinary method, has always proved fatal in the gangrene of ergotism,‡ and in the present instance I have no doubt that it would have been so. The poor fellow laid constantly upon his back, with the knees drawn up, and after the *left* leg had been removed he found much difficulty in placing the limb in a different position, nor did the sore go on so well as it had previously done. On this account, the right foot was detached by

■ "Sub morbi decursu reliquæ corporis partes satis bene valebant." Langius.

+ "Universa hæc malorum ilias pedisequam habet bulimum." J. A. Srine.

‡ "Quatuor mortui sunt post amputationem, gangrænâ ad truncum usque serpente." Noel, surgeon to the Hôtel Dieu, Orleans. "Amputatio mortem citat." Cl. Salerne, quoted by Tissot.



cutting through the ligaments, leaving about an inch of denuded bone above it. The advantage of this plan of treatment was very obvious. The limb remained in its customary position, and the sore healed rapidly to within a short distance of the projecting bone. With the stump in this state, the removal of the remainder of the bone at a future period will be attended with little risk, should it be deemed expedient. It is worthy of remark, that though the patient has kept his bed for so many months, and during the greater part of the time has lain upon his back, yet no bed-sores have occurred, with the exception of a very small one on the sacrum, which might have taken place in a healthy man confined for as long a time to the same position.

"The condition of this unfortunate being at the present time (November 12th, 1854), is as follows:—His general health appears to be good, and he has evidently gained flesh, eats and sleeps well, and is generally free from pain. The pulse is natural, and no morbid sounds can be detected in the heart. His strength appears to him to be restored, but his mutilated condition forbids him to test the truth of his opinion in this respect. There is no tendency to sweating, and the specific gravity of the urine is 1·017. It has an acid reaction, and shows no traces of albumen. The memory, both as regards recent and more remote events, is still feeble, which has added much to the difficulty of drawing up a connected statement of his case.\*

"The small sores on the nose and ears have perfectly healed. The thumb and forefinger of the left hand have separated at the middle of the first phalanges, and the stumps are cicatrized. It is remarkable that, in the right hand also, the thumb has separated, and the forefinger is about to separate, at the same points. The remaining fingers have all been detached at the joint between the first and second phalanges, leaving the former completely denuded for half their length. These subsequently became covered with granulations. Owing to the carelessness of the attendants, the fingers have been allowed to unite at these points. In the left leg, the bones of which were sawn through about seven inches below the tubercle of the tibia, the stump is still far from healed. The sore on the right leg has granulated as low down as the commencement of the epiphysis of the tibia. At this point, separation will probably take place. The flexors of the leg have contracted to such an extent as to render it impossible to straighten them."

Six months afterwards the patient is described as being in perfect health, fat, and florid.

The history of the family related by Dr. Wollaston is very remarkable, and we cannot refrain from copying this also from Mr. Camps' paper, particularly as it is essential to the confirmation of the diagnosis in his own case. The family lived at Wattisham, in Suffolk.

"The husband, wife, and six children were affected with this disease; the eldest being a girl of fifteen, and the youngest an infant of four months old. They were all healthy at the time of the attack. It commenced with severe pain, which in most of the cases attacked the left leg first, or as some of them described it, the leg and foot. The pain was so violent, that the whole neighbourhood was alarmed by the cries of the sufferers. In the course of a few hours the toes became much swelled, and after four, five, or six days, the pains abated, and the foot began to turn black, appearing at first covered with spots, as though it had been bruised. At this period, in those affected

\* "*Omnes ægri ab initio fere imbecilli, historiam morbi tradere nesciunt.*" Salerne quoted by Tissot.

in more than one limb, the other began to be attacked with similar pain. The swelling and discoloration gradually extended upwards, till, finally, the mortified parts separated from the bone, and, after a lengthened period, the bone itself was detached from its connexions. All, with a single exception, were affected about the same time, in the month of January, the weather being then warmer than usual. In the case of the father, the disease assumed a milder form, the fingers only becoming discoloured and contracted, and the nails of several of them falling off. For a long time afterwards, he continued to complain of darting pains in the legs, hands, arms, and back. One poor girl, attacked in the left foot, stood upon the other for three weeks, leaning against the chimney, till that foot also becoming affected, she took to her bed. During the whole time of this calamity, the family appeared in other respects well. They ate heartily, and slept soundly when the pains abated.

"At the termination of the disease, the father had recovered, with the exception of two fingers, which remained in some degree contracted. The mother, aged forty, had lost the right foot at the ankle joint, and the left leg a little below the knee; her hands, and part of her arms, remained with but little sensation, the fingers being also contracted. Mary, aged fifteen, had lost both legs below the knee, and was then dead. Elizabeth, aged thirteen, had also lost both legs below the knee. Sarah, aged ten, was deprived of one foot, and two of the toes of the remaining foot. Robert, aged eight, had both legs off below the knee; and Edward, aged four, had lost both feet at the ankle joints. The infant was weaned as soon as the mother was attacked, but became ill, and died in the course of a few weeks. It appeared to suffer violent pain, and the legs became black before death. Dr. Wollaston states that, with the exception of the mother, 'the rest of the family seemed well. One poor boy, in particular, looked as healthy and florid as possible, and was sitting on the bed, quite jolly, and drumming with his stumps.'

"No cause could be assigned for this dire visitation, except that of the family having lived for about a fortnight on bread made from wheat which had been badly harvested. Mr. Bones says, in reference to it—

" 'This wheat they have bought of the farmer whom I lodge with, who tells me, that last year he had some wheat *laid*, which he gathered and threshed separately, lest it should spoil his samples. Not that it was mildewed, or grown, but only discoloured, and smaller than the other. This damaged wheat he threshed last Christmas, and then this poor family used no bread but what was made of it, as likewise did the farmer's own family, and some others in the neighbourhood. A labouring man of the parish, who had used this bread, was affected with a numbness in both his hands for about four weeks from the 9th of January. His hands were continually cold, and his finger-ends peeled. One thumb, he says, still remains without any sensation.'

"Dr. Wollaston also says that the corn alluded to was very bad. 'It was wheat that had been cut in a rainy season, and had lain on the ground till many of the grains were black, and totally decayed.' "

#### ART. 23.—*Case of Scleroma.* By Dr. ROBERT M'DONNELL.

(*Dublin Hospital Gazette*, Feb. 1, 1855.)

This peculiar pachydermatous affection is but rarely met with in adults. It was described about ten years ago by M. Thirial, and

since this time a few cases have been put on record. Dr. M'Donnell refers to two of these cases in his paper.

CASE.—“ Mary Connell, a strong, robust-looking country girl, æt. 28 years, appeared among the patients at the Talbot Dispensary, August 11, 1854.

“ She complains of a hardness and rigidity of the skin over a great part of the body. Her appearance is peculiar. The skin of the face seems tightened and shining; there is a want of the power of expression in the countenance, the integument of which is so rigid as not to admit of the free movements of the brow and around the eyes and mouth, which are essential to an expressive countenance. She smiles or laughs with difficulty. As she says herself, her face feels stiff, as if it were coated with varnish or starch. The skin covering the neck, chest, shoulders, and arms, is in a similar hard and rigid condition: it feels not unlike the skin on a flitch of bacon. The mammæ are firm, and like brawn; the calves of the legs have a hardness like phlegmasia dolens. The abdomen and thighs are comparatively free from induration, with the exception of a patch the size of one's hand, above the right knee. The skin looks quite natural, except on the face, where it seems tense and shining, and browned, as if by exposure to the weather; it cannot be pinched up, nor even moved with freedom over the subcutaneous structures; this is most remarkable around the eyes, where the cellular tissue is in general lax. The peculiar rigidity seems to be seated in the subcutaneous cellular tissue, but does not depend on cedematous infiltration of it. There is no pitting upon pressure. The patient states that her general health has been, and still is, very good; pulse (which is not very readily distinguishable at the wrist through the indurated integument) 80; heart's sounds normal, and very distinctly audible; urine apparently normal, and *not* albuminous; no derangement of the menstrual functions. She complains of chilliness over the surface of the body, especially in the extremities; but, with this exception, she suffers from nothing more than ‘*stiffness of her skin,*’ and slight loss of flesh.

“ The complaint originated twelve months ago. It commenced in the neck and arms, and gradually spread to the other parts affected; for the last two months it has not become worse; the free motion of the joints is, to a slight extent, impaired; the wrists and ankles less stiff than the other joints. She does not suffer from globus hystericus, or other hysterical affections.

“ She was under treatment for more than a month, during which time she took bark and hydriodate of potass, and made use of a liniment of glycerine, distilled water, and hydrocyanic acid. Baths and unctuous liniments were also tried, but nothing seemed to be of the least benefit; and she ceased to attend at the dispensary December 17th, 1854.”



## SECT. II.—SPECIAL QUESTIONS IN MEDICINE.

## (A) CONCERNING THE NERVOUS SYSTEM.

ART. 24.—*Cases illustrating the Pathology of Mania and Dementia.*  
By Dr. A. J. SUTHERLAND, Physician to St. Luke's Hospital.

In this paper, which was read before the Royal Medical and Chirurgical Society, the amount of mixed phosphates in the urine is compared with the analyses of the brain and blood, and with the symptoms in cases of mania and dementia. The conclusions are based upon analyses of the urine, performed by Dr. Beale, in five cases of acute mania with paroxysms, four cases of common mania, two of intermittent mania, one of remittent mania, two of hysterical mania, three of puerperal mania, five of acute dementia, one of acute dementia with catalepsy, five of paralysis of the insane, three of chronic mania, and three of chronic dementia, making in all thirty-four cases. These results are compared with the analyses of the blood and brain by L'Heritier, Couerbe, Hittorf, and Michea.

The conclusions drawn from the observations are—

1st. That a plus quantity of phosphates exists in the urine in the paroxysms of acute mania.

2dly. A minus quantity exists in the stage of exhaustion in mania and in acute dementia, and in the dementia of paralysis of the insane.

3dly. The plus and minus quantities of phosphates in the urine correspond with the quantitative analysis of the brain and blood; for a plus quantity of phosphates is found and a slight excess of albumen in the blood of maniacal patients, and a minus quantity of phosphorus and albumen are found in the brains of idiots, and a minus quantity of albumen in the blood in paralysis of the insane.

4thly. The plus quantity of phosphates in the urine in cases of acute mania denotes the expenditure of nervous force, and is not a proof of inflammation in this disease.

ART. 25.—*On Meningitis in the Adult.* By Dr. J. LEWIS SMITH,  
Physician to the North-western Dispensary, New York.

(*New York Journal of Medicine*, March, 1855.)

This paper is an analysis of twenty-one cases of meningitis in the adult—this being the entire number of unequivocal cases (cases, that is to say which presented after death a lymphic deposit on or under the membranes,) which the author has been able to collect from various sources. In some of these cases the inflammation was primary, in others secondary, but when secondary the symptoms of the primary disease were not such as to produce any material modification in the inflammatory symptoms. The result of the analysis is to show

a considerable difference between the symptoms of meningitis in the adult and meningitis in the child.

*"Symptoms.*—Headache was one of the most common, generally severe, but sometimes slight. It is recorded in fourteen cases, in all of which it began the first day, and continued till the patients sank into delirium or coma. In no case is its absence recorded.

"One only had convulsions. This man was a soldier in the French army at the time of its retreat from Moscow, subsequently to which he was subject to epileptic attacks. An autopsy of all the viscera showed no disease except the meningitis.

"How the opinion has become so prevalent, that inflammation of the meninges gives rise to convulsions I do not know, but presume it is because this disease is most common in childhood, and convulsions usually attend this as well as other encephalic diseases in early life. Perhaps English and American physicians have derived their knowledge of diseases of the brain and membranes more from Abercrombie's treatise than any other source; and, as we have said, nearly all the cases in his collection were children. He gives the opinion that 'the more common form in which the attack takes place, is by a sudden and long-continued paroxysm of convulsions,' alluding to an attack of meningitis. On the contrary, our analysis clearly shows that convulsions are not a symptom of this disease except in childhood, and this correction should be made in our standard works.

"A rigid and flexed state of the upper extremities was present in one case, in one trismus, in another paralysis of one side of the face, in another of an arm, and in four of an entire side.

"Delirium was noticed in fourteen cases; in three coming on in the commencement of the disease, in the others not till near the close of life. It is not stated whether the remaining seven were delirious, so that if this symptom were present, it was probably of the passive kind.

"Vomiting, so common in the acute hydrocephalus of childhood, occurred in only six cases, and in these, with one exception, not till the disease was well advanced.

"The pupil in six cases was dilated during the comatose state, and in two others, before the development of coma, it was contracted, the condition during coma not being mentioned. Besides these, four exhibited some unnatural appearance of the eye, as strabismus, occurring probably from effusion. In the remaining cases the condition of this organ is not recorded. In one instance where the pupils were dilated, thirty leeches were applied to the neck, and while the bites were still bleeding contraction took place. This goes to show that simple congestion may cause dilation, which may not, therefore, be always so grave a symptom as is usually thought.

"Retention of urine was present in six cases, and incontinence in one.

"The pulse in seven was under eighty till near the close of life. Of these, three were phthisical, two had headache for two years, and one for life, one had had pain for a considerable time in the lumbar region, the cause not being apparent, and in the other the inflammation appeared to be primary. Three had a pulse varying from 80 to 100, two were phthisical, in the other the inflammation was primary. Three had a pulse over 100, of whom two were consumptives. The thought

may occur, whether this discrepancy in the condition of the pulse may not have been due to compression from the effused fluid. A compressed state of the brain will, in many instances, prevent acceleration of the pulse, though the inflammation is intense. But this explanation does not answer, for the symptoms of compression were generally absent till near the close of life. It is better to consider this diversity due to a difference in the grade of inflammation, as is the case in the inflammation of other serous membranes.

"The mode of death in sixteen cases is given, in all by coma, varying from a few hours to two or three days. Generally the effusion of serum and lymph seemed sufficient to cause the coma.

"The seat of inflammation in seven cases was the base of the brain, in four the convexity of one hemisphere, in three the upper surface of both, and in two the entire meninges. In the remaining cases the seat of disease was not recorded accurately, though the deposit showed undoubted inflammation.

"From this analysis the following conclusions may be drawn:—

"1st. That a common cause of meningitis is the tubercular diathesis.

"2d. That if in any of these cases the inflammation was primary, and not dependent on a diathesis, it did not differ materially from the secondary form either in gravity or duration.

"3d. That meningitis usually commences with headache.

"4th. That convulsions are not a symptom of it.

"5th. That delirium is present in the majority of cases, occasionally early, but generally not till the disease is far advanced.

"6th. Vomiting does not occur till a late stage of the inflammation, and then in only a moderate number of cases.

"7th. The pulse differs in different cases, and is, therefore, the less reliable as a means of diagnosis.

"8th. Paralysis sometimes occurs at a late stage of the disease, but generally there is no contraction or rigidity of the limbs.

"9th. That the mode of death is by coma."

ART. 26.—*On the nature and treatment of the cerebral symptoms occurring in Albuminuria.* By M. MARCHAL (de Calvi).

(*Gazette des Hôpitaux*; and *Dublin Hospital Gazette*, Sept. 15, 1855.)

The object of this communication is to show that these symptoms are the result of serous effusion either into the arachnoid sac, or into the subarachnoid cellular network, and not of uræmia, as has been supposed by *Frerichs* and many others. Hence, the author is of opinion, that there is much danger in the use of hot or of vapour baths in albuminous anasarca. He thus states what he calls "*his doctrine*:"—"When the albumen of the blood is escaping with the urine, whether in cases of pregnancy, or as the result of a chill to the surface, or after one of the exanthemata, especially scarlatina, or lastly, as in 'granular disease' of the kidney, the serum is apt to exude from the capillary vessels, the albumen being the bond of union between it and the rest of the blood; hence, dropsies arise. Under these circumstances, if owing to any exciting cause, as a vapour bath,



for example, or independently of this, owing to some idiosyncrasy, there be a determination of blood to the head, there will then be produced 'cerebral symptoms' of the convulsive, or comato-convulsive class."

The following case is given in illustration :

CASE 1.—"A *gendarme*, of robust health, with the exception of being slightly rheumatic, became suddenly chilled after having been in a perspiration. Next day he was admitted into the *Val de Grâce*, labouring under general anasarca; the urine was excessively albuminous; after being purged he seemed to be better. For the two next days, in accordance with the general advice of authors, he was given a vapour bath, when he was attacked with pain in the head, then with epileptic fits, in the ninth of which he died.

*Autopsy*.—"There was extreme hyperæmia of the kidneys, and pus was found in the parenchyma of the organ and in the pelvis. The brain was excessively congested, and there also existed serous infiltration of the subarachnoid cellular tissue."

A second example is then given, to illustrate further the danger of hot baths in such cases :

CASE 2,—was that of a young lady who was affected with albuminous anasarca during her pregnancy. She was very anæmic, and greatly enfeebled. At three months she miscarried. When consulted, M. Marchal examined the heart; it was healthy; the urine was albuminous, but only slightly so. He considered the dropsy was now chiefly owing to the extreme debility of the patient, and ordered iron, &c., whilst albuminous drinks, sweetened with sugar and eggs, were given to try and repair the loss of albumen in the blood. The patient was better, and had been able to go out to drive, when the ordinary medical attendant thought of giving baths to remove the dropsical effusions. The patient took three baths. Each time her face was a good deal flushed; after the third she suddenly lost her sight, and her speech became difficult, though her mind was unaffected. In a few days she died. There was no examination of the body.

ART. 27.—*On the differential signs of Cerebral Hemorrhage and Cerebral Softening.* By Professor TROUSSEAU.

(*Rév. Méd. Chir. de Paris*, May, 1855.)

On a recent occasion M. Trousseau made the case of a female patient, then in the Hôtel Dieu, the text for some practical remarks upon this subject. The case was this. The power of motion in one side was lost, and especially in the arm, but the intelligence and sensibility were unaffected. These symptoms had happened suddenly, and without any warning, a month previously. The patient had gone to bed, as she supposed, quite well, but in the morning she awoke with the arm benumbed. This was just before the menstrual period. For this symptom she caused herself to be bled, and during the bleeding the paralysis extended to the whole side. From this time until the next menstrual epoch she went on improving, but at this time she was again struck suddenly with complete paralysis of the same side. At this time she was immediately brought into the hospital.

What, then, was the nature of the case? The fact that both attacks had occurred at the time when the menstrual epoch was about to commence, and that they were sudden, and without any preliminary threatenings, suggested the idea of cerebral hemorrhage: but it might be softening. M. Trousseau inclined to the latter belief, but he did not venture to express a positive opinion.

Presently the patient died, and the inspection showed that M. Trousseau's suspicions were correct. There was decided softening in the brain, but no trace of hemorrhage.

The remarks upon the case are of considerable practical value. According to current opinion, cerebral softening cannot be confounded with cerebral hemorrhage. It is hemorrhage, we are told, when a person, without experiencing any precursory symptoms, is suddenly struck with hemiplegia, when he is suffering from loss of sensibility or consciousness. On the contrary, it is softening when (after various precursory signs of a convulsive and spasmodic character) the paralysis is developed gradually.

This, however, is not a constant rule. It is the rule, but there are exceptions. Hemorrhage may be preceded by warnings, and softening may declare itself suddenly. How, then, are these exceptional cases to be distinguished? Recamier gives the rule, and the rule is this: In hemorrhage there is agreement (consonnance) in the symptoms, that is, the three manifestations of cerebral activity, intelligence, sensibility, and the power of motion, are simultaneously affected; in softening there is disagreement (dissonnance) in the symptoms—that is, the power of motion is affected without any corresponding affection in the intelligence and sensibility.

According to this diagnostic law, then, the patient under consideration was attacked with softening, and not with hemorrhage, because her intelligence and sensibility were unaffected; and the result, so far as it goes, proves the correctness of the law. At the same time it was natural to suspect hemorrhage in a case where the symptoms occurred so suddenly, and at the time, moreover, when the menstrual flow was about to commence.

ART. 28.—*Case of softening of the Cerebellum.*  
By Mr. ROBERT BIANCHI.

(*Lancet*, Feb. 24, 1855.)

In this case there was partial loss of muscular power in the lower extremities, and marked excitement of the sexual function.

CASE.—W. R.—, æt. 65, was formerly a porter in the Borough Market. During his occupation he used to drink to a great extent, and was altogether irregular in his habits. Between fourteen and fifteen years ago he became an inmate of St. Saviour's Union Workhouse, having been compelled to leave his employment on account of a gradual loss of muscular power in his lower extremities. Since his admission into the workhouse, various remedies (amongst them strychnia and electricity) have been tried, without the slightest benefit; and although he did not get worse, still there was no improvement. He has always been able to walk about by means of a stick, but if he at-

tempted to hurry, directly fell down. When spoken to quickly, his legs would start, and remain in a state of agitation some minutes. About seven months ago he began to have incontinence of urine, and since that period has almost entirely remained in the same ward. He went to bed, no alteration being noticed by the other inmates of the ward in his state of health, and was found dead in the morning. Whilst living in the workhouse, his extreme partiality to the female sex has been remarked, and also that he was constantly addicted to masturbation. (These facts I have ascertained since making the post-mortem examination; and also that his wife had not lived with him for two years, while he was able to get his own living, on account of his want of constancy.)

*Post-mortem Examination, fourteen hours after death.*—The body generally was rather emaciated, the scrotum and thighs excoriated, and the testicles extremely small; both lower extremities much wasted. The cerebrum, after a very careful examination, appeared quite healthy. On attempting to remove the cerebellum, I found it impossible to do so, the whole substance breaking down on touching it, although before attempting its removal the proper form was preserved, and the grey matter was still distinguishable from the white. The medulla oblongata was quite healthy. On examining the chest, I found a large abscess in the right lung, but could not trace any bronchial tubes into it, which would account for its not presenting itself as a symptom during life. The left lung was perfectly healthy. The heart was a good deal encumbered with fat; the liver in a state of cirrhosis; the kidneys showed granular degeneration. There was thickening of the bladder generally; and a small abscess had formed by the side of the rectum.

#### ART. 29.—*On the diagnosis of Tumours within the Cranium.*

By Dr. FREIDREICH.

(*Beiträge zur lehre von dem Geschwulsten innerhalb der Schädelhöhle*, Würzburg, 1853; and *Edinburgh Monthly Journal*, March, 1855.)

Dr. Freidreich has recently published a very interesting monograph on this subject, containing the particulars of forty-five cases of intracranial tumours, eleven of which were observed by himself; and on these he has founded some valuable remarks as to the mode of their diagnosis. The following is a brief resumé of his observations on these points.

1st. *The General Effects of Intracranial Tumours.*—Various derangements of sensibility occur. One of the most constant of these is *headache*, which is especially frequent in the early stages of the disease. The cephalalgia is remarkable for its persistence and intensity; it may be either continuous or intermittent; and it may be accompanied by *vertigo* and *vomiting*. Its site does not always indicate the place of the new growth. The organs of special sense may be affected; the *sight* becomes impaired on one or both sides; there may be *strabismus*; and the *hearing* generally suffers more or less. The minor grades of *paralysis* are common. The length of interval between the initiatory headache, and the occurrence of the paralysis constitutes the most characteristic mark of these intracranial tumours. *Convulsions* and *spasmodic conditions* occur in one half of all cases; and the former often assume an epileptic type. The *mind* is always



more or less affected; its diseased condition generally commencing with *loss of memory*.

All these symptoms are very inconstant and variable; they are also liable to alternate remissions and exacerbations, which probably are due to the occurrence of transitory congestions either of the tumour, or the cerebral substance, or perhaps of both together. The course of intracranial tumours is always chronic. Friedreich never knew a case to be shorter in duration than six weeks, or longer than fourteen years.

2d. *The Special or Differential Diagnosis of Intracranial Tumours.\**

—(a.) Those situated in the *cerebral hemispheres* (18) are generally accompanied by obstinate headache (14); nausea and vomiting (9); by derangements of the motory functions (14); consisting of more or less extensive paralysis, and of convulsions which assume an epileptiform character. When hemiplegia occurs it is sometimes crossed (*gekreuzt*), and sometimes not; but it constantly occurs on the affected side. Derangements of the special senses are common (10), especially of sight (7); and intelligence is often impaired (11). In a few exceptional cases there are no headache or alterations of the motor functions.

(b.) *Tumours of the base of the cranium in the neighbourhood of the pons*, occasion the following symptoms:—(9) headache, (8) almost always frontal; impairment of vision (7), commonly also of hearing and taste (5), and in some cases (3) of smell. All these symptoms, due to loss of power of the facial nerves, occur on the same side as the tumour; but paralysis of the extremities, when it occurs, affects the opposite side of the body. *Complete hemiplegia and paraplegia* are not very common; and convulsions occur less frequently than with the former class of tumours, and are not epileptiform. An important sign of these tumours is afforded by the great multiplicity of the existing sensorial disturbances, and the tendency of the optical derangement to become bilateral. The mind is sometimes affected (5).

3d. *Tumours of the Pituitary Region*.—Friedreich only saw one case of this. There was frequent frontal headache, often with pain in the orbit; and double amaurosis. There is rarely any disturbance of the motor functions.

4th. *Tumours of the anterior part of the Base of the Brain*.—Two cases were examined. The symptoms resemble those just mentioned.

5th. *Tumours of the Peduncles of the Cerebrum and Cerebellum*.—Paralysis of the face and extremities, occurring on the side opposite to that on which the tumour was situated, was observed in three cases. This makes tumours thus situated resemble those of the hemispheres. Complicated derangements of the nerves of special sense and of the face (as the oculo-motor and trifacial), were seen in two cases. This, on the other hand, approximates these tumours to those of the base.

(f.) *Tumours of the Cerebellum* (8) had the following signs:—Violent cephalalgia (7), often intermittent, and combined with vomiting (4); and situated in the occipital region (4). *Occipital*

\* The numbers placed above in brackets refer to the number of cases, among those examined by Friedreich, in which the particular symptoms were present.

headache may be considered to be pathognomonic here, as it occurs in the case of no other intracranial tumours. Pain at the nape of the neck, increased by pressure, may exist. In one case there were no peculiar symptoms; and in none of the cases of tumours of the cerebellum were the generative functions at all affected.

(g.) *Tumours of complex situation.*—Correct diagnosis is here impossible.

ART. 30.—1. *On the condition of the Muscles in Hemiplegia.*

By Dr. J. RUSSELL REYNOLDS.

*With some comments by Dr. MARSHALL HALL, F.R.S.*

(*Lancet*, Sept. 8,—22, 1855.)

In our opinion, Dr. Reynolds appears to make out his case very well in this very interesting and valuable paper. This, however, is not Dr. Marshall Hall's opinion, as the criticism which we append will show.

(A.) Three different statements being on record with regard to the irritability of muscles paralysed in consequence of cerebral lesion, Dr. Reynolds has made a series of observations, in order, if possible, to solve the following questions:—

1. Do the paralysed muscles differ in irritability from those of the healthy side?

2. If differing, what is the nature of that difference?

3. Are the alterations observed of the same kind in all cases, or are they variable?

4. If variable, can they be referred to appreciable conditions of the limbs in question, such as the state of nutrition, of contraction, &c.?

5. Or are they referable to variations in the mode of experimenting?

6. Have the conditions of irritability any, and what, value in pathology and diagnosis?

“Through the ready kindness of my friend Mr. William Filliter, I have been permitted to examine these questions in the cases of twenty hemiplegic patients under his care in the St. Marylebone Infirmary; and I am also indebted to Mr. Filliter for most valuable assistance in the prosecution of the inquiry.

I. The *cases examined* are twenty in number, and are all examples of hemiplegia, differing in the amount and duration of paralysis, and also in the conditions (of contraction, nutrition, &c.) with which the paralysis is associated. These points are related in every case, but in this paper a summary of them will alone be given.

II. The *mode of examination* has been the same in each instance, the tests for irritability being of two kinds—percussion and the galvanic current.

1. *Percussion.*—This is an exceedingly ready test of irritability, and the differences observed between the healthy and paralysed limbs, in

obedience to its application, are most obvious in the greater number of cases. Very little force is required, a gentle tap over the belly of a muscle being sufficient to elicit marked contraction. The muscles have been examined in groups—viz., the flexors and extensors of the forearm, hand, and fingers. Each group has been compared with its corresponding group on the healthy side, and care has been taken to avoid confusion with reflex spasms by observing the results of pinching the skin, or percussing it over a bone.

2. *Galvanism*.—The galvanic current has been applied by means of a Cruikshank's battery of 50 plates, each being rather less than two inches square. The exciting fluid employed consisted of dilute nitro-sulphuric acid, in the proportion of one in sixteen parts. The mode of applying the current was as follows:—*a*. The feet of the patient were placed in one basin of water, and the hands in another, a wire from the battery, terminating in a copper-plate, being connected with each basin. The order of observation was the following:—First, the current was directed from the feet to the arms—i. e., the “direct” current (as Matteucci termed it, or that current which takes the course of the motor nerves, from the trunk towards the extremities) was observed, and contrasted in the arms. Each arm was examined separately—i. e., placed in the basin by itself, in order to ascertain the lowest number of plates which would cause an appreciable contraction of the muscles. Then the two hands were immersed equally in the water, and a similar observation made as to the lowest number of plates causing contraction in each, and as to the relative force of the contractions in the two arms, with low and high powers. Secondly, the order of the currents was inverted, and a similar series of observations made, with the current “inverse” or “indirect” (to the motor nerves) in the arms. *b*. A second series of observations were made with the current directed from arm to arm—i. e., direct in one and inverse in the other. These several observations were recorded at the time, and the degree of irritability of the two sides, judged of from the number of plates required to induce contraction, and the relative distinctness and force of that contraction.

It was, at all times, endeavoured to place the two sides in perfectly correspondent conditions; and in order to estimate the amount of variation which might follow from an unequal immersion of the hands or feet, experiments were made upon a healthy adult; the result of which was that such inequalities as might have occurred were altogether inconsiderable; for, when intentionally exaggerated to the highest point, the differences produced did not amount to more than that of one plate.

### III. *Results of examination.*

#### 1. Those obtained by percussion :

No difference between the two sides in . . . . .	6 cases.
Paralysed side the more irritable in . . . . .	14 „

Of these 14 cases, 3 presented slight contraction of the muscles (upon percussion) on the healthy side; in the remaining 11 there was none. In 5 cases, the difference between the two sides was very considerable,



the irritability on the paralysed side being notably increased. Of the first 6 cases the two sides were equally unaffected by percussion.

2. Results obtained by galvanism :

No difference between the sides in . . . .	2 cases.
Healthy side the more irritable in . . . .	13 „
Paralysed „ „ . . . .	5 „

In no one case was the irritability extinct ; and it is extremely curious to observe that the same cases presented marked differences in respect of irritability as tested by the two methods. In order to answer the fourth question proposed at the commencement of this paper, the three groups of cases, separated by the results of galvanic examination, will now be compared.

First. *The 13 cases exhibiting a diminished irritability on the paralysed side.*—Of these, 8 were males, and 5 were females. The paralysis affected the right side in 5, the left side in 8.

The duration of paralysis was—

From two to nine months in . . . .	4 cases.
„ one to three years in . . . .	6 „
„ three to four years in . . . .	1 „
Above ten years in . . . .	2 „

The amount and extent of paralysis were as follows :

	Fingers.	Wrist.	Forearm.	Arm.
Completely immoveable in . . . .	10	9	7	2 cases.
Partially moveable in . . . .	3	4	6	11 „

Tonic contraction of the flexor muscles was observed to be distributed thus :

	Fingers.	Wrist.	Elbow.	Shoulder.
Absent in . . . .	2	3	2	7 cases.
Slight in . . . .	1	1	4	4 „
Highly marked in . . . .	10	9	7	2 „

The sensibility of the paralysed limbs was—

Normal in . . . .	6 cases.
Diminished in . . . .	3 „
Exalted in . . . .	1 „
Not examined (not recorded) in . . . .	3 „

Nutrition of the muscles (judged of by their size, their firmness, and the appreciable temperature of the limb) exhibited the following variations :

Normal condition in . . . .	7 cases.
Slight wasting in . . . .	4 „
Considerable wasting in . . . .	2 „

The results obtained by percussion were that—

The paralysed limb was unaffected in . . . .	4 cases.
„ „ the more irritable in . . . .	9 „
And of these 9, very highly irritable in . . . .	4 „

Secondly. *The 5 cases exhibiting a relative increase of irritability on the paralysed side.*—Of these, 4 were males. The paralysis was situated on the right side in 3, on the left in 2.

The duration of paralysis was—

From one to five months in . . . . .	2 cases.
„ three to eleven years in . . . . .	2 „
Many years in . . . . .	1 „

The degree and distribution of paralysis were as follows :

	Fingers.	Wrist.	Forearm.	Arm.
Completely immoveable in . . . . .	4	4	2	0 cases.
Partially moveable in . . . . .	1	1	3	5 „

Tonic contraction existed in some cases, and was absent in others, thus :

	Fingers.	Wrist.	Elbow.	Shoulder.
Absent in . . . . .	2	2	1	3 cases.
Slightly in . . . . .	1	1	1	1 „
Highly marked in . . . . .	2	2	3	1 „

Sensibility was normal in 2 cases, diminished in 3. Nutrition was normal in 4 cases, whereas slight wasting was observed in 1.

The results obtained by percussion were that—

The paralysed limb was unaffected in . . . . .	1 case.
„ „ the more irritable in . . . . .	4 cases.
And of these 4 cases, highly irritable in . . . . .	1 case.

Thirdly. *The 2 cases exhibiting no difference between the sides.*—Both were women. The paralysis affected the right side in each; and had existed for many years. There was complete loss of motility in one, incomplete in the other. Contraction was slight in each. The muscles were wasted in one case; nutrition was normal in the other. Sensibility normal in both. Percussion elicited no contraction in one; but distinct irritability in the other.

IV. *Conclusions.*—From these observations we may make the following inferences in reply to the questions stated at the outset :

I. That in some cases of hemiplegia, but that not in all, the paralysed limbs do differ in irritability from those of the healthy side.

Hence, 1. That hemiplegia (cerebral paralysis), *per se*, does not necessarily involve any change.

2. That such changes as may occur are due to subsequent, or co-existent, but additional morbid processes.

II. That the differences observed are in the degree of irritability.

III. That these differences are not constant; in some cases being those of excess, in others those of deficiency.

Hence, that the processes which affect the irritability of the muscles in hemiplegia are of different kinds.

IV. That the variations observed cannot be referred to either the sex of the patient, the side affected, the amount or duration of paralysis, to changes of sensibility or nutrition, or to the co-existence of tonic contraction.

V. That all the differences observed cannot be referred to varia-

tions in the mode of experimenting; since, in all the cases here recorded, the mode of examination was precisely similar.

With regard to this fifth conclusion it may be remarked, that during the course of these experiments certain observations were made which illustrate two possible sources of fallacy. (1.) In every case the direct current was considerably more energetic than the indirect, (a fact previously stated by Matteucci); and, in some instances, the difference between them was greater than that between the irritability of the two sides. When this was the case, the direction of the current determined the side in which the more energetic contraction should occur. This source of fallacy affected only the second series of observations (*b*); but it was observed in both classes of cases, and could be readily rectified by the first series of experiments (*a*). (2.) It was observed in several cases where the irritability was in excess on the paralysed side, as tested by the low power, that a higher force induced more energetic contractions on the non-paralysed side. This illustrates Dr. M. Hall's statement, that the former is alone a test of *irritability*; the latter, an index of contractile *power*; and this appears to have been overlooked by some observers.

VI. That the pathological inferences are somewhat complicated, and their relation to diagnosis of negative character.

*a.* With regard to pathology, these experiments show—

1. That we may distinguish between the nervous and muscular irritability of hemiplegic limbs. This appears to be warranted by the different results of galvanism and percussion. The phenomenon, contraction, is the same in each instance, and there is every reason to believe that it is in all instances the special property of the living muscle. What is different is the mode of stimulation. It is generally admitted that percussion acts immediately upon the muscle itself; whereas the differences observed between the effects of the direct and indirect galvanic current, and the notable want of conformity between the galvanic and percussion experiments, render it evident that the former represent something different from, or in addition to, the state of muscular irritability, and make it probable that (as Dr. Todd suggested) "the excitability of the paralysed muscles to galvanism varies with the condition of their nerves more than with that of the muscles themselves."\*

2. That the muscular and nervous irritability are affected in different directions; the one being increased and the other diminished in the same cases.

3. That, as a rule, the nervous (galvanic) irritability is diminished. This occurred in 13 of 20 cases, and the conclusion is in accordance with that of Dr. Todd. At the same time the observations recorded are at variance with Dr. Todd's mode of accounting for the fact—viz., that "the contractility or irritability of paralysed limbs bears a direct relation to their state of nutrition."

4. That in some cases the nervous irritability is increased. This conclusion, warranted by 5 of 20 cases, is in accordance with that of



Dr. Marshall Hall ; but the existence of a larger number of cases presenting the opposite condition indicates that Dr. Hall's statement is not universally correct.

5. That in rare cases the nervous irritability is unchanged. This condition was noticed in two cases only ; but, according to M. Duchenne, it is that which may be most frequently observed.\* With regard to M. Duchenne's statement, it may be remarked that, although the existence of these two cases indicates that hemiplegia *per se* does not necessarily involve any change in the condition of electric irritability, yet that the absence of some change is the exception and not the rule.

6. That in the majority of cases the muscular (percussion) irritability is augmented in hemiplegia. This conclusion, warranted by 14 of 20 cases—i. e., by all which exhibited any change from the normal condition, is in accordance with the general proposition of Dr. Marshall Hall, who was the first to invite attention to the subject. But the galvanic observations, if correctly interpreted, are at variance with Dr. Hall's explanation of the phenomena—viz., “that the spinal marrow is the special source of the power in the nerves of exciting contraction, and of the irritability of the muscular fibre.”†

7. That in some cases (6 of 20) there is no evidence of increased muscular irritability.

8. That in all cases in which the muscular irritability was found increased, the augmentation (being on the paralysed side) was constantly associated with diminished volitional power ; but, on the other hand, the loss or diminution of volitional power does not necessarily induce any increase of muscular irritability.

9. That the existence of increased muscular irritability may be, but is not necessarily, associated with the augmentation of nervous excitability.

10. That the alterations of muscular irritability are not referable to conditions of nutrition, to the presence or absence of tonic contraction, to the state of reflex excitability, or to the amount or duration of paralysis.

b. With regard to diagnosis, it may be concluded—

1. That no positive change of irritability must be expected as a means of distinguishing cerebral from spinal paralysis ; but

2. That the negative evidence afforded by the absence of notably diminished irritability may be of service in diagnosing paralysis which results from exclusion of the cerebrum only, from that which is owing to exclusion of the spinal cord.

(B.) In commenting upon these statements Dr. Hall says:—Whenever I have repeated my experiments on the condition of the irritability of the muscular fibre in paralytic limbs, on a considerable number of patients, I have met with *exceptional* cases. This fact I have recorded on several occasions.

I noticed this event again particularly on my last trials at the St. Marylebone Infirmary in 1854. I called Dr. Reynolds' attention to

\* *Traité de l'Electrisation Localisée*, Bull. Gén. de Ther., t. xlvii, p. 340.

† *Medico-Chirurgical Transactions*, vol. xxii, p. 205.

the fact, and begged him to investigate the subject, and I think the profession greatly indebted to him for the deeply interesting results communicated in the 'Lancet' of the 8th instant.

I think all who have experimented on this subject, have agreed in one conclusion: *the irritability is diminished in spinal paralysis*, or that paralysis which, from whatever cause (such is my definition of the term), excludes the influence of the spinal marrow.

It is in regard to cerebral paralysis that the difference of opinion has existed. I, with the exceptional cases to which I have alluded, and which I myself discovered, have found the *irritability* in this case *augmented*, comparatively, at least, with that of the other limb, if not positively; I believe the latter, though *not* in the *degree* in which it is diminished in the other form of paralysis.

The interesting question is—What is the explanation of the exceptional fact? For Le Gallois has a beautiful paragraph—

“However opposed results may appear, we must remember, that of two facts, both well established, one can never exclude the other, and that the apparent contradiction depends on some intermediate element which has escaped us.”

What is the “intermediate element” in regard to the question before us, in regard to which there is, as in all such cases, if the inquiry be pursued, a *discovery* to be made?

I believe I have made that discovery.

It consists in the fact that all cases of *hemiplegia* are *not* cases of *cerebral paralysis*.

These are not convertible terms. The very title, therefore, of Dr. Reynolds' admirable paper is objectionable. Cerebral paralysis is *one* thing; hemiplegia, *two* or more.

For example: In a severe case of hemiplegia we meet with *two* series of phenomena. The first consists in paralysis of volition in the arm and leg of the side opposite to that of the lesion; this is *cerebral* paralysis. But there is a second. There are frequently stertor and dysphagia; these are symptoms of *spinal* paralysis. Further, the arm and leg themselves are sometimes so affected by spinal *lesion*, or by spinal *shock*, as to be at once affected by *cerebral* and by *spinal* paralysis. In such cases the phenomena of reflex action, and not the phenomena of reflex action only, but the irritability must needs be impaired, according to the *law* which I have discovered.

This state of things becomes chronic. These severe and chronic cases, amongst the poor, are consigned to the workhouse. It is in the workhouse, therefore, that we meet with these *exceptional* cases chiefly.

In my first experiments my patients were chiefly private patients, and I particularly selected cases of distinct, but not too severe character—of moderate, but not too long duration—of decided, but not absolute loss of voluntary power, and I only met with *one* exception to the law of augmented irritability in cerebral paralysis.

In my subsequent experiments, I made no selection of cases, and there were then *several* exceptional cases.

In my last experiments, already alluded to, made in the institution in which Dr. Reynolds made his, again I met with *several* exceptional

cases, although the number of my experiments was small, the fact which I mentioned to Dr. Reynolds, with the request which I have already noticed.

As to this idea of *exception*, although we use the term, I need scarcely observe that it is utterly erroneous. There *can* be no exception to a physiological law. *An exception is an error—an ignorance.*

Thus there can be no exception to the laws—1, That in pure cerebral paralysis there is augmented irritability; 2, That in spinal paralysis there is diminution of this irritability.

But when we speak of hemiplegia, we use a term which does not always signify the same thing; which, in fact, sometimes signifies *cerebral*, and sometimes *spinal* paralysis, and the phenomena are in accordance with the *things*, not with the *word*.

The first error committed in this investigation, consisted in the wrong choice of an instrument. It was one of too great violence. The *power* and not the irritability of the muscles was tested.

The second error consists in the wrong choice of terms and of cases. Hemiplegia is sometimes purely cerebral paralysis; sometimes combined cerebral and spinal—a fact in some degree new, resulting, as usual, from a new course of inquiry.

Cleared from all sources of ambiguity in the mode of experiment and in the use of terms, I repeat that my two laws in regard to the condition of the irritability of the muscular fibre in paralytic limbs remain—

1. In pure cerebral paralysis—that in which the influence of the cerebrum *alone* is removed—there is augmented irritability.

2. In spinal paralysis—that in which the influence of the spinal marrow is *also* removed—there is, and in a far greater degree, diminished irritability.

As corollaries from these facts become laws, I may repeat that the difference in the irritability of the muscles of the healthy and paralysed limbs in cerebral paralysis is the difference between muscles moved by volition and muscles unexcited; whilst the difference in the irritability of muscles of healthy limbs and limbs affected by spinal paralysis, is the absence or presence of the very *source* of this irritability, and of the other physiological conditions of these muscles. The difference in the *degree* of augmentation in the first case, and of the diminution in the second, is, therefore, perfectly intelligible to all.

To these statements may still be added the conclusion, that, duly employed, the galvanic current becomes a diagnostic—and it has often been a *corrector* of the previous diagnosis—between cerebral and spinal paralysis.

It is trying to think that, after such distinct annunciations of the truth, we shall next read of paralysis—the *cause* of which is *seated* in the cerebrum, or in the spinal marrow, respectively, as synonymous with cerebral and with spinal paralysis. Whereas a disease *seated* in the cerebrum may *extend its influence* to the spinal marrow, and produce spinal paralysis; and a disease not seated in the spinal marrow at all, but in the course of a spinal nerve, may, and does also, produce spinal paralysis, whilst a disease seated in the spinal marrow may produce cerebral paralysis.



ART. 31.—*On Cerebral, Spinal, and Ganglionic Paralysis.*

By Dr. MARSHALL HALL, F.R.S.

(Lancet, Sept. 29, 1855.)

One great result has flowed from the investigation into the varied condition of the irritability of the muscular fibre in paralytic limbs—the fact, that *hemiplegia* is sometimes *cerebral*, sometimes *spinal paralysis*—sometimes *limited* to the exclusion of the influence of the cerebrum, sometimes *extended* to the exclusion of the influence of the spinal marrow.

The distinction which I have established in regard to these two forms of paralysis, to which in this paper I add a third, is anatomical and positive.

When physicians speak of *hemiplegia*, they in reality use a term the signification of which has reference merely to a *symptom*; and that symptom may have a double or even a triple origin.

If hemiplegia affects and excludes the influence of the cerebrum only, the case is, I repeat it, cerebral paralysis; but if it affects or excludes the influence of the spinal marrow also, as it does in some severe cases, it is spinal paralysis; it will constitute one of those cases which, from our ignorance of their real nature, and from our error in viewing the terms cerebral paralysis and hemiplegia as synonymous and identical, have been regarded as *exceptional* cases.

These exceptional cases are rare amongst the milder cases of private practice; amongst the severer cases consigned to the workhouse, they may amount, as in the subjects of Dr. Reynolds' inquiries, to three fourths of the whole number of cases.

If our terms be once well-defined, all ambiguity is removed: cerebral paralysis excludes the influence of the cerebrum only; spinal paralysis that of the spinal marrow also. The characteristics of each of these, when they are themselves distinct, are as fixed as the laws of physics.

To cerebral and spinal paralysis I must add a *third*, viz.—*ganglionic* paralysis. This paralysis is excluded in pure cerebral paralysis; it is included in spinal paralysis.

Thus, in cerebral paralysis the muscles become *atrophied*; in spinal, in reality also ganglionic, paralysis, they become *heterotrophied*, if for distinction, I may use that term. I have long regarded the ganglion on the posterior roots of the spinal nerves as parts of the true ganglionic system.

Thus, again, in cerebral paralysis the irritability of the muscular fibre is *augmented*; in spinal paralysis it becomes gradually more and more *diminished*; in ganglionic paralysis, if complete, it may become *extinct*.

In both an anatomical and in a physiological sense, the muscles in cerebral paralysis remain muscles, and their irritability, being unexhausted by the stimulus of volition, is, *pro tanto*, augmented, compared with that of the healthy limbs; whilst in spinal paralysis they gradually lose their muscular power, and in ganglionic paralysis they

cease to be muscular, either in structure or in function. In certain cases, as M. Cruveilhier and M. Duchenne have shown, the muscular fibre undergoes the fatty degeneration which has recently attracted so much attention.

After these explanations and definitions, I think our investigations may proceed without any of those apparent exceptions and contradictions which have so much obstructed our progress. We must bear them continually in mind; and we must distinguish between true irritability and mere *force*, and the results will be uniform (unless, indeed, some other element of complication exist, still undetected); and all difference of opinion, so discreditable to physiological and medical science, will cease.

I will now, for the sake of still greater distinctness, throw the subject into a tabular form.

I.—*In Cerebral Paralysis—*

1. *The Reflex Actions,*
2. *The Influence of Emotion, and*
3. *The Influence of Strychnine,*
4. *The Irritability,*

*are more noticed in the paralytic than in the healthy limbs;*

II.—*In Spinal Paralysis—*

1. *The Reflex Actions,*
2. *The Influence of Emotions,*
3. *The Influence of Strychnine, are extinct, and*
4. *The Irritability diminished.*

III.—*In Ganglionic Paralysis—*

1. *The Structure, and*
2. *The Functions, may be alike destroyed.*

Cerebral paralysis may exist alone. Spinal paralysis, of course, implies cerebral paralysis. Ganglionic paralysis may exist with or without spinal muscular paralysis. In division or disease of the trifacial nerve we have ganglionic paralysis. And in a case which I formerly published, in which the digital nerve being injured, the nail ceased to grow as formerly. But as spinal paralysis implies cerebral paralysis, it also implies ganglionic paralysis. I have at this moment an interesting patient, who, from inflammation of the sciatic nerve from cold, has lost the power of the limb; the muscles are absolutely unaffected by galvanism, atrophied, heterotrophied, and, I suppose, changed into fat. By restoring the healthy condition of the *nerve*, will the morbid change of structure undergo restoration? This is a question never yet agitated. It will require much observation and—*experiment*, to determine it satisfactorily; and I propose shortly to add to the present brief sketch some ample details.

I shall first add the enumeration of some other forms still to paralytic affection.

ART. 32.—*Case of "Paralysie Musculaire Progressive."*  
By Dr. THEODOR VALENTINER.

(*Vierteljahrsh. für die Prakt. Heilk.*, 1855; *Medico-Chir. Rev.*, Oct., 1855.)

The primary lesion, in this interesting case, appears to have been in the spinal cord. (We copy Dr. Sieveking's abstract of the case).

CASE.—"A gentleman, æt. 45, of robust and athletic habit, and peculiarly gifted, had always enjoyed good health; and although he had occasionally committed excesses in his cups, was generally temperate, and remarkably fond of gymnastics. Ten years previously he once fell on his back, on a sandy soil, from a height of eight or ten feet. About two years after this, the patient, who had experienced no inconvenience at the time of the fall, thought his health failed; his florid complexion became sallow. A year later, in 1847, he had a slight attack of pleurisy; but as late as 1852, he could have experienced no great diminution of physical strength, for he was still able to carry two fifty-pound weights in each hand. In April, 1853, he first consulted Dr. W. H. Valentiner, who found that he had then very little power in his hands; that he was unable to exert any forcible compression, or stretch out his fingers completely; the right hand was the weaker of the two; no emaciation was perceptible; there was a difficulty about all the movements of the body, and the patient had a difficulty in conveying his food to his mouth; no tenderness or change of any kind was perceptible in the spine; no cerebral affections could be traced. The various physicians consulted regarded the affection as one of the nervous system, without being able to localize it. The patient was sent to Franzensbad, a Bohemian watering-place, where he used the baths, and then went to Nancy, in the south of France. Dr. T. Valentiner (the author) was now consulted, and having become acquainted with Cruveilhier's cases, diagnosed the present one as an instance of fatty degeneration of the muscles, with atrophy of the anterior roots of the spinal nerves. In the autumn of 1853, though the paralysis became more and more marked, some hopes were excited in the patient by the occasional occurrence of sudden and frequent twitchings in the affected muscles. In November, the patient was conveyed back to his home; he then exhibited extreme emaciation of the upper extremities; none of the limbs could be properly extended; in an attempt at walking, the feet dragged on the ground; his back was almost bent double; the face, though already showing symptoms of paralysis, still retained its intellectual expression; sensibility continued unimpaired in all parts of the body. At last, dysphagia supervened; and although the appetite continued good, the paralysis progressed, and a slight attack of bronchitis, in March, 1854, terminated the patient's misery.

"The following are the main results of the post-mortem examination:—The deltoids and other muscles of the upper and fore-arm had almost disappeared; the muscles of the hands were entirely converted into fat; the muscles of the cervical and lumbar region presented a tolerably healthy appearance, but in the dorsal region they were pale, and traversed by yellow bands; and one fasciculus closely resembled fat. The microscope confirmed the various degrees of fatty degeneration observed by the naked eye. The muscles of the lower extremities were in a healthier state than those of the upper. On slitting up the entire dura mater of the spinal cord, about one hundred small white bodies were observed scattered over the dorsal surface of the cord, exclusive of the cervical portion; varying in size from a pin's



head to a small pea, smooth externally, rough on the inner side, and grating under the knife; under the microscope they exhibited a dense fibrous tissue, interspersed with pigment. All the anterior roots of the spinal nerves were distinctly thinner and smaller than the posterior roots, flabby, resembling a tissue filled with a reddish serum, and exhibiting under the lens a marked vascularity; healthy nerve-fibres were still visible under the microscope, but many were in a state of fatty degeneration. Nothing of the kind was found in the posterior roots; the cord was found abnormally soft at the part where the three lower cervical and four upper dorsal nerves are given off; at this part the distinction between the grey and white matter was almost effaced; numerous glomeruli (granular corpuscles) were found in the softened part; they occurred in the white, as well as in the grey, substance. Scarcely any well-marked ganglionic cells were discoverable in these parts, but were found in the unsoftened portions lower down; no marked lesion could be discovered in the brain; the heart, healthy to the naked eye, showed under the microscope incipient fatty degeneration; much oily matter was contained in the hepatic cells; the nerves of the brachial plexus exhibited no abnormalities; excepting congestion of some parts of the lungs, and slight pleuritic adhesions at the right apex, no other visceral lesion was discovered."

ART. 33.—*Case of Paralysis of the third pair of Nerves.*  
By Dr. CHAVANNE.

(*Archiv. Gén. de Méd.*, May, 1855.)

This case is remarkable for an extraordinary phenomenon—effusion of blood in the eyelids. Age not mentioned.

CASE.—M. T—— consulted M. Chavanne on the 17th June, 1853, for acute pain in the antero-lateral part of the right side of the head. There was disgust of food, frequent nausea, and not unfrequently fits of great lowness of spirits. The sight was painful, the pupils slightly contracted, and that on the right side less irritable than the other. Pulse 56. Leeches were applied to the anus, sinapisms to the feet; and he was directed to rest, and take a bottle of Seidlitz water on the morrow.

18th.—Better. The pain has now fixed upon the infra-orbital region, and changed into the character of tic.

21st.—Signs of cerebral congestion, with great depression of spirits.

28th.—Considerable tumefaction of the eyelid of the right eye, with dark discoloration as from the copious effusion of blood into the tissues. This came on spontaneously in the course of the night. It was difficult to separate the lids; but when this was done, the eyeball and the conjunctiva appeared to be in a natural state. The pain ceased with the appearance of the swelling. The general health was not at all affected.

July 1.—On examining the eye, M. Chavanne was astonished to find a state of strabismus, by which the eye was turned outwards and upwards. The pupil, also, was slightly dilated and motionless; the sight manifestly enfeebled; the eye somewhat prominent; vision double.

Some days later the eyelids had nearly returned to their natural condition, only retaining some slight traces of ecchymosis, but the power of raising the upper eyelid was very defective. This fact, indeed, and the strabismus before mentioned, gave evidence of paralysis in the oculo-motor nerve of this side.

During the next three months there was a steady and progressive improve-

ment; and at the end of that time every trace of paralysis had disappeared from the eyelids, the eyeball, and the eyelids; and the patient was restored to his former sound condition.

ART. 34.—*Cod-liver Oil in the treatment of Neuralgia.*

By Dr. DURANT, Physician to the East Suffolk and Ipswich Hospital.

(*Assoc. Med. Journal*, Oct. 6, 1855.)

"I was induced," writes Dr. Durant, "to make trial of this remedy as a last resource in a case of the most severe facial neuralgia which I have ever witnessed. The pain was so severe that it amounted to agony, the tears involuntarily flowing over the cheek. Iron, quinine, and arsenic, in full doses, had severally failed in affording more than mere temporary relief. The oil had not, however, been taken longer than a week when the pain became sensibly diminished; and, by a perseverance in its use for a few weeks, the disease, which had existed for many months, was perfectly cured.

"This patient continued well for two years, when he again became the victim of a similarly severe attack. Recollecting the failure of other remedies in his former seizure, he begged that he might at once commence the oil, which desire was at once acceded to, and was attended with a like happy result. From the satisfactory termination of this case, I have been induced to prescribe cod-liver oil very largely in many forms of neuralgia, and, upon the whole, with decidedly good effect.

"Among the more severe cases in which I have seen cod-liver oil act with especial benefit, I may enumerate one of ocular neuralgia, a very severe case of neuralgia of the tongue, many cases of facial neuralgia, several obstinate cases of sciatica, and two or three cases of neuralgia of the rectum, the exquisite suffering from which I have found it both speedily and permanently relieve. Indeed, in this latter form, whether in combination with, or independent of, hæmorrhoids, the oil appears to act most beneficially, and I can with great confidence recommend its use.

"Since noting these memoranda, I find the value of cod-liver oil in neuralgia fully corroborated by Dr. Theophilus Thompson, in his valuable *Clinical Lectures on Pulmonary Consumption*."

ART. 35.—*On the treatment of Sciatica by Croton Oil.*

By Mr. HANCOCK, Surgeon to the Charing Cross Hospital.

(*Lancet*, April 28, 1855.)

The six following cases are related for the purpose of proving the position, already advanced by Mr. Hancock ('Abstract,' xix, p. 176), that sciatica, in the majority of cases, depends upon mechanical irritation of the pelvic nerves, from loaded intestine, or tumours within the pelvis, and not from rheumatism or inflammation of the nerves, as is commonly supposed. The cases certainly show the value of the treatment adopted.

**CASE 1.**—Captain C—— consulted me on the 30th of December, 1853. Has been ill twelve months, with pain in the lower part of the back, extending down the right hip, increased by fatigue. He attributes the pain to a fall on the flat of his back, at which time he had symptoms of ague, rigors, total loss of appetite, great weakness, and nausea; profuse sweating, with bad nights. He had stiffness, like lumbago, after stooping, for above eighteen months. His bowels, at present relaxed, act regularly once daily, but if balked, they become confined; he sleeps badly; his appetite is moderately good, but very capricious; is easily tired, but becomes refreshed after a short rest. He had a slight attack of yellow fever six months ago. Condition of urine natural; pain relieved by pressure; he is most easy whilst lying on one side (either), with knees raised; he feels blown out after eating. Take of croton oil, one minim; mercurial pill and extract of henbane, of each four grains; and compound extract of colocynth, eight grains: mix, divide into four pills; take two every other night.

January 13th, 1854.—Better; the medicine acted very powerfully; he felt better directly afterwards; has had much to do, but has been so free from pain as scarcely to notice it. To take five grains of compound aloes pill occasionally.

**CASE 2.**—C. A— consulted me, in November, 1853, for sciatica of the right leg. He has been ill four months, and treated for rheumatism by colchicum, blisters, &c., without benefit; he is bent nearly double with pain extending down the course of the sciatic nerve and in the lumbar region; pain most severe at the knee, foot, and calf of that leg; his appetite is good, but he gets no sleep, and feels very weak. Ordered the aperient pills, with croton oil.

The patient came again in four days; he says he was much better the day after the first dose, but, contrary to my advice, he repeated the pills on the following night, which acted so violently that he felt knocked up, the pain being very severe. To have three grains of disulphate of quinine, three times a day.

I saw no more of this patient, but on the 2d of January, 1854, his brother, who consulted me for a similar complaint, informed me that he had rapidly recovered under the use of the quinine.

**CASE 3.**—H. A— consulted me, on the 2d of January, 1854, for sciatica of the right side, of five months' duration. He complains of pain in stooping extending down the sciatic nerve, but most severe at the calf of the leg; he cannot straighten his leg with comfort; urine plentiful, but thick; appetite good; bowels rather relaxed. Ordered the aperient pills, with the croton oil.

January 6th.—Pain better in the back, but very severe at the calf of the leg and tuber ischii; complains of sensation of pins and needles in his foot. The medicine acted freely, producing watery evacuations. To have five grains of compound aloes pill every night.

10th.—Better; pain less severe in the back; has still numbness in the heel; urine more clear. The medicine has acted twice daily, removing solid, dark, pulpy, and scybalous matter. Repeat the aloetic pills every night. To have two grains of disulphate of quinine twice a day.

14th.—Well.

**CASE 4.**—E. C— consulted me July 24th, 1854, for sciatica of right side, of five years' duration; he has never been free from pain during that time. Has taken thirty grains of quinine daily, colchicum, arsenic, hydrocyanic acid, calomel to salivation, and is now taking the iodide of potass. Has had blisters and various irritants applied, also a hot iron passed down the leg in the course



of the nerve. When at the worst he became deaf, but has now recovered his hearing; was galvanized for nine weeks. He has suffered from habitual constipation, and occasional diarrhœa. Having read my paper in the 'Lancet,' he took the remedies advised therein, and has been better since, but he does not attribute any benefit to them, having occasionally been better, and then relapsed; he is better for change of air. The complaint commenced with lumbago. He has difficulty in passing his urine, and when in great pain cannot micturate without straining for half an hour; his pulse averages 72. For the last twenty years coughing or sneezing has produced pain down the sciatic nerve, and eight years ago he suffered so severely from sciatica, that in walking he was obliged to assume the sitting position for two or three minutes before he was able to walk again. He now complains greatly of a sensation of weight in his loins, and states that his sufferings are so great, particularly at night, that he cannot obtain ease without taking frequent large doses of opium. Ordered two of the aperient pills, containing croton oil, every other night.

August 4th.—Mr. C— has taken two doses of the medicine, both operating very powerfully; the secretions were dark-coloured and mixed with scybalæ. He has slept better the last two nights, and has had more ease in walking, with less pressure upon the back; but the pain always returns in greater violence about half-past nine in the evening. He has taken twenty-five drops of the bimeconate of morphia for the last two nights, and has slept comfortably; he can pass his urine with less difficulty. To have three grains of disulphate of quinine three times a day.

18th.—Has repeated the aperient pills of his own accord with similar results, and has since taken the quinine. He has not had such acute pain of late, and the pressure upon the back is removed; but he has still much pain as soon as he gets to bed every night, and some flatulency, but the latter he thinks is in some measure relieved. He is still obliged to have recourse to opium. To repeat the quinine, and to diminish the quantity of opium, with a view to its discontinuance.

September 5th.—He has continued the quinine to this date; has had no return of pressure upon the back, but complains greatly of flatulency, particularly when in bed; he has better nights, having rarely had recourse to opium, which he had been obliged to take almost every night for some weeks. He says, however, that at different periods he has been able to abstain for a few weeks. His bowels now act regularly without aperient medicine.

December 18th.—I have not seen the patient since last report. To day I received a letter stating that there had been no return of pressure upon the back; that he generally sleeps well, having only found it necessary to take opium two or three times, and that for extreme flatulency; that he considered the sciatica cured. He finds that when he dines out, and takes more stimulant, that he has less flatulency. Has discontinued the quinine for three weeks. To have three grains of compound aloes pill every day before dinner.

February 7th.—Has derived great benefit from the last prescription; does not now take opium, and sleeps well; can now bear the motion of a two-wheeled carriage, which he has not done for five years.

March 1st.—Mr. C— called upon me to day; he is cured.

CASE 5.—Mr. W. K—, æt. 52, residing in Essex, was sent to me in October, 1854, having for years experienced weakness in his back and numbness down the right leg. At present he is suffering from lumbago and pain in the right leg, of six weeks' duration. He caught cold, and was attacked with lumbago, for which an embrocation, a warm bath, and blister

were employed, with sufficient relief to enable him, in the course of a fortnight, to resume his occupation of a farmer for nine days. He was not cured, as walking and riding caused him great pain; he could not ride on horseback. At the expiration of this time he again caught cold, and he became worse, the lumbago being more intense, and the pain extending along the gluteal region down the thigh to the knee. Another blister was applied, and tonic medicines prescribed, with some little benefit. He says his bowels are easily acted upon; has suffered from internal piles, and experienced considerable languor for three weeks before the attack, also weakness of his back and chilliness, and a sensation of fulness after eating; perspires greatly. He has had ague several times previous to fourteen years ago; but he draws a distinction between his present chilliness and that complaint in the absence of the hot stage; during the present illness flexing the right thigh on the pelvis has been effected with difficulty and pain, referred to the back of the pelvis, and latterly a gland in the groin has enlarged. He and his family being very anxious lest he should be suffering from lumbar abscess and spinal disease, Mr. Warwick, the gentleman under whose care he was, sent him up to me, and I had the satisfaction of confirming that gentleman's opinion, that there was no mischief of that nature, but that Mr. K— was suffering from lumbago and sciatica, depending upon loaded colon. To have two pills, containing the croton oil, every other night.

November 2d.—Mr. K— called upon me to day very much improved. He took six pills, and found himself so much better, and the pills had acted so freely, that he discontinued them. He has not taken any medicine for three weeks. He is now quite free from pain, is able to stoop and ride on horseback for a short distance, and can hold himself upright, which he could not do when last here. Describes the alvine evacuations as lumpy, and as though there had been a collection. Repeat one of the pills occasionally; to have three grains of disulphate of quinine, in the form of pill, three times a day.

Not having seen or heard of this patient since the last report, I conclude that he is now quite well.

CASE 6.—Mr. A. H— consulted me on the 1st of August, 1854, for sciatica of the right side, of twelve months' duration. He had tried the Harrowgate and Cheltenham waters, and cold water cure, without effect. His tongue is much loaded, and complains of itching at anus, and bleeding piles. He says he keeps his bowels open by taking cold water night and morning. He has applied very strong stimulating liniments to the leg without benefit. He has suffered from gout. Ordered, spirits of turpentine, two drachms; castor oil, half an ounce; add cinnamon-water to an ounce and a half; make a draught, to be taken at bed-time.

August 7th.—The medicine acted very powerfully, and with decided relief to the pain. It is now dull, like that of a bruise. To have an ounce of the compound guaiacum mixture twice a day.

I have not seen this gentleman since the last report, he having returned home into the country; but his cousin, who consulted me a short time afterwards, informed me that he was quite recovered.

ART. 36.—*Case of hysterical Hydrophobia.*

By Professor BURGGRAEVE, of Gand.

*(American Medical Monthly, June, 1855.)*

This case is an interesting addendum to the article on Hydrophobia in our last volume (XXI. p. 252.) It is another argument in favour of the view there taken, and it is all the more valuable because as yet few of these cases have been recorded.

CASE.—A man, æt. about 50, of a nervous temperament, was brought to the Civil Hospital of Gand, labouring under characterised fits of hydrophobia. He had never been bitten, and knew not what to attribute his disease to. The fits returned at intervals, nearer and nearer, and in a truly frightful manner. The patient began to experience at the epigastrium a constriction, which soon extended to the pharynx, and rendered deglutition not only difficult, but painful. Hence the very idea of drinking distressed him exceedingly. When they offered him the vessel to quench the thirst that burnt him, he clung to it with rage, and by dint of violent efforts hardly succeeded in swallowing a few drops of water. The eyes were sparkling, and shunned the light; every shining body increased his agitation. The tongue presented on both sides of the frenum the two small spots observed in ordinary cases of hydrophobia. The patient fell at last into a state of cerebral congestion, which ended in death.

The autopsy did not reveal anything particular about the brain or the meninges, except the injection. The back of the mouth and the pharynx were red, and the latter strangely contracted. At the lower end of the œsophagus existed a ball of lumbricoid worms, some of which had ascended the tube. M. Burggræve thinks these worms may have caused the symptoms under which the patient died. If we analyse these symptoms, says he, we find an hysterical condition carried to its extreme violence. The irritation of the œsophagean nerves extended to the pharynx, and produced there the hysterical ball or constriction. He thinks that if the cause could have been suspected, a vermifuge or a simple emetic might perhaps have saved the patient.

ART. 37.—*On the diagnosis of Epilepsy.* By Professor TROUSSEAU.*(Medical Times and Gazette, Aug. 11,—25, 1855.)*

Although this terrible disease was known in ancient times, we seek in vain for any exact description of it until the year 1825, when M. Calmeil produced his remarkable thesis. A pupil of M. Rostan's at the Salpêtrière, he lived during a year among the epileptic patients, and observed with great sagacity several thousand attacks. The thesis consists only of some twelve or fourteen pages, but it is a little masterpiece. His not having had the opportunity of studying the disease as seen in private practice, explains the omission of one of its forms very frequently met with, and to which M. Trousseau directs particular attention.

The form described by the Latin writers, under the name *morbus comitialis*, *morbus sacer*, and *herculeus* is the "grand mal" or "fit," characterised by the following signs:—An acute cry, prostration of



the patient, who falls to the ground, convulsions of a special form and short duration, which are followed by a state of somnolence that lasts a longer time, in proportion to the amount of cerebral congestion present. A minute acquaintance with each of these symptoms prevents a physician ever failing to recognise epilepsy when it exists, while its simulation cannot be successful unless when performed by a skilful medical practitioner. Esquirol, indeed, maintained it could never be simulated; but one day, at Charenton, while he was familiarly discussing the cases with his pupils in his study, he observed M. Calmeil fall on the carpet, the subject of alarming convulsions. M. Esquirol having examined him with great anxiety, turned round exclaiming, "The poor lad is epileptic." The words were hardly uttered, when Calmeil sprang up, and grasping his master's hand, asked him if he was still incredulous as to the simulation of epilepsy.

Nevertheless, there is a sign present at the instant of the fall which no one can imitate, viz., a most marked, cadaveric pallor, that for an instant occupies the entire face. The practitioner arrives usually too late to witness this, the face having by that time become very red. After the fall come the convulsions, but not instantly, there being almost invariably, prior to their advent, a very short period of complete immovability. A marked character of these epileptic convulsions consists in their predominance on one or the other side. Sometimes, though this is rare, they are observed only on one side, but in no case are they equally strong on the two sides. During the first or tonic stage you observe the thumb bent upon the palm, the pronators of the forearm, and the rotators inwards of the arm, enter into permanent contraction, and turn the arm inwards with a slow and interrupted motion, that is sometimes violent enough to induce luxation. At the same time the head is turned to the opposite side, by the contraction of the sternocleido-mastoidean of the side on which the predominance prevails. Ignorant of this, simulators do not fail to twist it to the side on which the convulsions of the arm are strongest. The violent convulsion of the muscles of the face causes a deviation of the commissure of the lips and of the eyes to the side where the predominance exists, giving rise to horrible grimaces. During this period the walls of the chest and abdomen are rigid and motionless, respiration is suspended, whence arise engorgement of the venous system and congestions, and frequently there is involuntary emission of urine, fæces, or semen. The contraction of the depressor muscles of the lower jaw keeps the mouth half open, while the tongue, through the action of the genioglossi is thrust out between the teeth. The bloody foam which results from its being bitten, is a great aid to diagnosis, especially when the attacks are only nocturnal.

When this tonic contraction has lasted from ten to sixty seconds, the clonic convulsions appear, very rapid at first, but becoming separated by wider and wider intervals of relaxation. After they have continued from one to two minutes, they cease completely, and the patient, now in a complete state of resolution, heaves a deep sigh, lets his head fall down, and exhibits a stertorous respiration like one struck with apoplexy or dead drunk. To this first attack there frequently succeeds a second, a third, and so on; and it is to this often fatal con-

dition that the term "grand mal" is applied at Bicêtre and Salpêtrière.

In the simple attack, the coma lasts for eight or ten minutes, when the patient arouses, looks around with anxiety, and places his hand to the forehead as if to recall something that he had forgotten. He seems as if ashamed, seeks to withdraw himself from the observation of the bystanders, neither answering their questions, or thanking them for their services. Moreover, we may remark not infrequently, or even ordinarily, signs of a true derangement of the intellectual faculties. Some patients have been seized with a desire of suicide and have even executed it, while others exhibit acts of violence towards those who are present. A considerable number manifest hallucinations or true mania; but frequently headache and sadness are all that remain after the attack. Some of these patients have embarrassment of speech, owing to the painful and swollen state of the tongue, but you must be on your guard here, and not lightly pronounce, as many do, the commencement of a general paralysis. During the existence of the convulsions and coma, the patient is absolutely insensible. This is of importance in diagnosis: for you may excite the mucous membrane of the eye and ear, allow ammonia to be breathed, or fire a pistol in the ear, without the truly epileptic manifesting the slightest sensibility. The simulator, too, chooses the place where he is to fall and the part of the body that is to bear the shock; and we never find him falling face forwards, without irresistibly interposing his hands as a protection. The epileptic falls no matter where, and he usually does so upon the head, his face exhibiting ecchymoses and wounds in consequence.

Epilepsy is very often a nocturnal disease, especially at the commencement; and so it may continue for eight or ten years without any one, not even the patient himself, being aware of the existence of so important a malady. There are two principal diagnostic signs in such a case, viz., the biting of the tongue, and the involuntary emission of urine, especially in women. If the person who comes to consult you complains of waking with headache, if the lateral parts of the tongue are lacerated, and if you can ascertain that urine has been passed unconsciously, do not hesitate to declare that there has been a nocturnal attack of epilepsy. Moreover, in a very great number of cases, you may observe on the forehead, and especially below the eyes, myriads of petechiæ the size of a pin's head, which are never produced under other circumstances. In possession of these details, the diagnosis of this form of the disease becomes certain, while without their aid, it is almost always impossible.

*Epileptic Vertigo.*—This is often met with in practice, and its existence is as real as is that of the epileptic fit, like which, too, it affects the intellect. It is scarcely possible to describe it, save by examples. In childhood, when it is especially common, it may manifest itself thus:—The child stops short in the middle of its play, remains motionless, with fixed eye and suspended respiration, returning to itself after seven or eight seconds, and sometimes hardly two. We may observe analogous examples in the adult. A person while playing at cards finds the movement of his hand suddenly arrested when about to play, the card remaining in his hand, as if affixed to it. A deep inspiration



occurs, the suspended movement is completed, and the vertigo has passed away. At other times the patient rises, walks he knows not where, striking against objects, and stops short at the instant he returns to himself. At others, he mumbles some unintelligible words, or repeats the same word, as his own name, obstinately, during seven or eight seconds. In all these cases the individual is completely without the external world. Sensation is abolished, and we may shake or pinch him without his feeling anything. In certain cases, as in a patient now in the wards, the vertigo is announced by a peculiar sensation, to which authors have given the name of *aura*, and which, in the great majority of cases, consists in the feeling of a current, that mounts up from one of the limbs, or some other point of the surface, towards the head. At other times there is a sensation of pain, of formication, or of little imperceptible convulsive shocks. In a great number of cases these phenomena constitute the entire affection, and deserve the name of epileptic vertigo. At others, they go on increasing until the fit itself occurs, and then it is usually by the thumb that the aura commences. But the fit is only preceded by the aura quite exceptionally.

A child, five years of age, was brought for M. Trousseau's advice. Several times a week, and sometimes more than once in a day, the child became the subject of hiccough, which, accompanied by remarkable paleness, lasted for several seconds, and never more than a minute, headache and hebetude succeeding. M. Trousseau, alone in his opinion, pronounced this epilepsy, and a year after the child had regular epileptic fits. At other times, epilepsy manifested itself by a marked sensation of cardiac suffocation. The patient, seized with most violent palpitations, becomes extremely pale, and loses all consciousness. In ordinary palpitation, consciousness is always preserved; and it is well to be aware of these palpitations in the epileptic, since the patient complaining only of his heart, an erroneous idea of the nature of the disease may be easily formed.

I have stated that disturbances of the intellect are very frequent after the epileptic fit, and they are also met with after vertigo. The head is heavy and aching, the patient being morose and taciturn, and as if stupified for a while, as a half or whole hour. For the purpose of diagnosis, it is of extreme importance to observe these changes; for we find them as a consequence of no other nervous spasm, however violent it may have been. There may be exhaustion after a violent fit of hysteria, but the intellect always remains very clear. This relative confusion of the mental powers may escape the physician's attention, but it is very rare for it to escape that of the patient or his relatives, so that they should be always interrogated upon this point.

There is nothing special in the vertiginous form, as it depends upon the same causes as the fit; and very often we observe alternations of the vertigo and the fits in the same subject. It is by no means rare, however, to find, after from one to ten years' time, the fits entirely displace the vertigo.

*Diagnosis from Hysteria.*—I will suppose you observe one young girl the subject of slight vertigo, which scarcely lasts for three seconds, and then another rolling about on the ground in every direction, uttering cries, breaking every object near her, and requiring to be held



down by four or five persons—men by preference. It might seem to you that the first had little the matter with her, and that the other was very ill. Not so; the first is the dog that bites without barking, the other the dog that barks without biting.

In hysteria there are usually precursory symptoms. The subjects of it complain of a sense of suffocation, of a fulness about the stomach, an indefinable nervous irritation, and of the sensation of a ball rising into the throat. During the attack the movements are extensive, powerful, and irregular, both sides of the body participating. Considerable change of place results, and much force is required in order to restrain them. There is much noise and little danger; and the scene terminates with a peculiar cough, sobs, tears, and the emission of aqueous urine. The epileptic is a far more quiet patient. Where he is struck down there he remains motionless, and if it should chance to be in the fire he may be burned to ashes. The primary tonic contraction is replaced by clonic contractions, small in extent; there is an amount of insensibility never observed in hysteria, and at the end of the attack peculiarities are noticeable that have already been described.

*Diagnosis from Eclampsia.*—As to all the phenomena proper to the attack, these are identical; so that we may call eclampsia epilepsy, wanting the relapse; and epilepsy eclampsia with relapse. This is true only as regards the form of the affections, for between their nature there is as much difference as there is between gout and a swelling of the great toe from a prick. In children, we frequently observe eclampsiform convulsions, and they have been regarded as symptomatic of rubeola, variola, and scarlatina, &c. I think that is somewhat of an error; nevertheless, at the onset of a rubeola, &c., the child has a slight convulsion, preceded by a little cry, and throwing back of the head; and a minute after he returns to himself, and takes the breast. Such convulsions usually cease of themselves, and if treatment be adopted for them death will usually be the result. If the convulsive state is reproduced, we usually observe what the nurses call “inward convulsions.” The infant is panting, closes and reopens its eyes, a slight guttural *râle* is heard, and the child becomes red and goes off to sleep again,—it is an epileptic vertigo.

However much the convulsions observed during the attack of epilepsy and eclampsia may resemble each other, their continuousness in the latter presents a ray of light for the diagnosis. The attack of epilepsy is of short duration, and when the attacks succeed each other so rapidly that there is not time enough for the patient to recover from the stupor of one before another comes on, we still observe that, whenever the *carus* commences, the muscles cease to be convulsed, and fall into a state of complete resolution. Such cessation is not observed in eclampsia. During one, ten, twenty, or thirty hours, the patient remains with the eyes convulsed, the head thrown back, and the limbs rigid, without any period of *carus* with resolution manifesting itself; so that we may define eclampsia as, and it is an excellent sign of distinction, a continuous tonic or clonic convulsion. Nevertheless, it does occur, though very exceptionally, that epilepsy assumes this continuous form in children. It then depends upon some cerebral

lesion, as, for example, tubercles, which become the cause of a cerebral phlegmasia, amidst which the convulsions assume the character of continuousness characteristic of eclampsia.

The two affections are, indeed, so nearly allied to each other, that we too commonly see eclampsia transformed into epilepsy. Thus, we find children suffering from frequent convulsions during dentition; then, again, on the occurrence of some disease, as rubeola, visceral phlegmasia, etc.; after some years these recur from some insignificant cause, and at last without any cause at all. These children, at first eclamptic, have become epileptic. Moreover, in the families of epileptic patients, convulsions or eclampsia are of frequent occurrence. As a practical rule, when you see convulsions in a child accompanying dentition or an acute affection, do not be needlessly uneasy; but when they occur towards the fifth or sixth year from the slightest cause, and especially without any cause at all, you should entertain the greatest fear that the child is epileptic.

ART. 38.—*On the inter-paroxysmal condition of Epileptics.*

By J. RUSSELL REYNOLDS.

(*Lancet*, Aug. 4 and Aug. 11, 1855.)

On examining a large number of epileptics Dr. Reynolds says we shall find no difficulty in recognising the existence of the three following groups broadly separated from each other:

1st. There are those who themselves say, and whose friends confirm the statement, that they are in perfect health.

2dly. There is another group, marked by notable mental failure, although various in both kind and degree.

3dly. There are others who present some marked derangement of the general organic health, or of some particular functions.

"Of 71 cases which have fallen under my own observation, I place 29 in the first class, 31 in the second, and 11 in the third. These are respectively equal to 40, 43, and 15 per cent.

"Some of the cases placed under the second head presented deterioration of general organic health; but so far as I could learn this was the result of circumstances arising since the development of the disease.

"In examining those of the *first class* more closely, it is found that in respect of intellectual endowments and of organic strength they are (as they say) in perfect health. Many of them are remarkable for their intelligence, quickness of apprehension, memory, and good sense; at the same time their nutrition, temperature, and muscular strength are natural; they eat well, digest well, sleep well, feel well. But even in these individuals there are to be discovered some peculiar phenomena, decidedly not those of health. These are often attended with so little inconvenience that the patients make no spontaneous complaint of their presence, or they may even not recognise them when asked the question, although the physician has no difficulty in their detection. The phenomena to which allusion is made are common to the whole

series of epileptic patients ; but they alone exist in the first class, and will therefore be considered now.

“The first of these is muscular *tremor* (found in fifty per cent.) It is either constant or occasional, and when the latter, is induced by mental, emotional, or organic changes, or by external impressions, slight in degree, as compared with the intensity of the result. This tremor is variable in extent and in intensity, passing from tremulousness (unnoticed by the patient, but readily detected by the physician if he grasps one of the limbs) to well-marked rigors. The occurrence of rigors is by no means uncommon in epileptics, when they make any intellectual exertion, are subjected to emotion, or to any physical change.

“But passing beyond this condition, some *clonic spasms* are exceedingly frequent ; they occur in 57 per cent. Variations are observed in the situation and force of contraction. In some cases they have not attracted the patient’s observation ; in others they are very annoying, but, so far as I have seen, never painful. In this respect these contractions differ widely from the tonic spasm of common ‘cramp.’ When occurring in the neck, they give rise to an unpleasant sense of constriction, the patient frequently feeling that his cravat is too tight, although this may not in reality be the case. (Trachelismus of Dr. Hall.) In the limbs, they cause jerking movements : if the hands and arms are affected, objects are thrown out of the former ; if in the legs, the patient may fall down. This occurred seven or eight times in the day in a gentleman under my care. If sitting, the legs would be suddenly extended with much force ; the diaphragm was sometimes affected, and then an unpleasant barking sound was caused by a sudden expiration. In other cases, the spasm assumes a tonic character, the limbs, head, and neck, being fixed for some moments, and the patient unable to move or speak. This may occur without the slightest obscuration of mind. In one curious case which I have seen, there were not unfrequently attacks, resembling, in respect of the convulsive elements, a perfect epileptic fit, but there was not in these attacks any loss of consciousness. Strabismus is frequently observed ; and when this evidence cannot be obtained, it may be inferred that irregular action of the ocular muscles takes place, from the patient complaining of occasional diplopia. There is, in children, a peculiar oscillation of the eyeball. Carpopedal contractions and grinding of the teeth belong to the same category. Starting from sleep is exceedingly common, and although in many instances this may be due to some emotional, or semi-volitional condition connected with dreams, in other cases it does not appear to be so related. In several children I have noticed a more or less constant condition of choreic movements, these being much exaggerated for some days prior to an attack. Further, the reflex movements, of physiologic character, are often abnormally performed ; deglutition is clumsy ; respiration hurried or jerking, often suspirious or yawning. A peculiar stertor is very common in epileptics, even when awake. These phenomena, taken together, were marked in 52 cases, or 73 per cent., and it is probable that the number would be much larger if the patients could be observed more constantly, as they were very often denied until



discovered by my own direct observation. Our knowledge of disease is accurate in proportion to the number of objective symptoms. Those the description of which passes through the patient's mind, are subjected to the moulding influences of his individual peculiarities, and that which makes us differ each other in intellectual life, makes us describe differently the conditions in which that life is placed, or by which it is affected. \* \* \* \*

"In addition to these muscular phenomena, we frequently recognise in the first class of epileptics an excessive readiness of emotional disturbance, and an absence of power for the control either of the emotion itself, or of its expression. The degree to which such excess of emotional action may be carried is very variable, and it is presented much more frequently by women than by men; and in the former the epileptic attacks sometimes alternate with, are preceded or followed by, hysteric phenomena. \* \* \* \*

'The *second class* of cases is formed by the presence of more or less marked change in the intellectual powers. This field is so wide that I can do little more than mention some of the most prominent objects it contains. By far the commonest and earliest change is *loss of memory*. At first it is noticed only with regard to the trivial matters of the day; whilst those long since passed are readily recalled. Subsequently, the patient forgets the earlier elements of his knowledge, and his mind then becomes an utter blank. The progress of deterioration resembles, in many respects, that which is natural to the decay of human life; often, as it were, anticipating the work of time, and hurrying a just opening life into a premature old age, with all its feebleness, and more than all its gloom. Failure of memory (except when occurring only as the immediate sequel of severe attacks) is commonly attended with diminished power of apprehension; and this is at first most marked with regard to new ideas, but, subsequently, appears to affect the mind in relation to previous knowledge, diminishing the power of applying past experience to the new circumstances of daily life. The patient cannot, or frequently does not, concentrate his thoughts upon any subject. Ideas follow one another, it may be in very rapid succession, as they are accidentally suggested by one another, or by surrounding events. When this power is only slightly deteriorated, the mind may be recalled by a strong effort, or a powerful impression; but when the intellectual disease has advanced further, this becomes impossible, and incoherence of expression indicates but too plainly the incoherence of thought, which may pass still further into utter fatuity.

"These earlier mental changes resolve themselves mainly into defective volition. The first failure of memory is due to want of attention rather than to anything else. The individual does not sufficiently attend to what is going on for deep impressions to be made, and consequently there is no power which can recollect them. Attention appears to be simply the direction of consciousness by an effort of volition; and in this first failure there is the first indication of diminished will. Probably the loss of apprehension has its origin in the same cause; it is the consequence of neglected or not properly exercised attention. By simple disuse, the power becomes diminished.

The same thing is to be observed with regard to thought. The associations, which in mental health form the basis of correct judgment and logical appreciation, from having their ground in the truest relations which we can discover between separate ideas, are lost altogether, or are replaced by associations of a merely accidental, or inessential character; and thought becomes incoherent, or "wandering," from the deficiency of voluntary power exercised in its direction and control. Thus, with deficient volition, and with increased readiness of emotional disturbance, the epileptic is reduced to a mere machine played upon by every external impression, or suggested feeling, and without any power to appreciate, account for, or control his state.

"It may be said that some epileptics give evidence of most powerful and determined will. History furnishes the record of some of the world's masters who were epileptic, and I have seen some cases marked by great so-called self-willedness and determination; but these appear to be exceptions, and to be more readily referable to obstinate or pertinacious clinging to an idea, for which very frequently the individual can supply no reason. It is, as he says, an impression which guides him, not a duly-formed volition, and an impression which he will admit to be as frequently wrong as right. There are doubtless some cases in which no abnormal deficiency can be detected; but many individuals, in whom excess of determination might be at first sight supposed to exist, in reality possess a volition defective in its highest function—viz., the control and direction of their own minds.

"The question at once arises, with regard to the failures which have been mentioned, whether they are not simply the consequences of attacks? In order to answer this question, let us remember the large class of cases in which no such mental deterioration could be discovered; and yet farther, that the degree of intellectual weakness bears no constant proportion to the severity, frequency, or duration of the fits, as the following numbers show. They represent the first twenty cases of which I have the record, and they are taken, therefore, indiscriminately. Eight presented marked cerebral failure; twelve did not. Of the eight in whom the memory and other faculties were deteriorated, the disease had existed from one to five years in four, from five to ten years in three, from ten to fifteen years in one; whereas of those in whom the mind was healthy, the disease had existed from one to five years in three, from five to ten years in two, from ten to fifteen years in three, from fifteen to twenty years in one, from twenty to twenty-five years in two, and upwards of forty years in one. The question of mental failure is not to be resolved, then, into one of duration of the disease. I have examined this question very carefully in all the cases which have fallen under my notice, and the result is, that in almost every case in which the mind was severely impaired, this impairment was noticeable from, or very soon after, the commencement of the disease, and increased steadily as the disease advanced; and the probability is, that those who have not after a few attacks evinced any mental failure, will not do so to any marked degree, even after many years have passed. In some cases, the mind had suffered seriously before any convulsive phenomena presented themselves. In one case, ridiculous ideas, and even distinct delusions,

existed for nearly two years, at the end of which time a convulsive paroxysm occurred, and the patient has since become a confirmed epileptic. In another, occasional aberration occurred for six months, and was then followed by two or three epileptic attacks, from which, however, and from his mental disturbance, this patient has now happily recovered. In a third case, decidedly maniacal attacks took place, not either immediately before, or immediately after, the fits, but in the intervals between them, and sometimes they appeared to alternate with the convulsions. Instances of variations might be multiplied indefinitely, but I feel convinced that when an epileptic presents mental failure or derangement, it is not so much the result of the paroxysms to which he has been subjected, but the effect either of the epileptic conditions themselves, or of some morbid processes developed coetaneously with them.

"The *third class* is formed by those patients in which there is some evident bodily change. This change is partly of a physiological character—the first and second dentition, puberty, pregnancy, parturition, and the climacteric period,—and partly pathological—plethora on the one hand, anæmia on the other; and gathered around these two conditions almost every form of local disease and of general cachexia." About this class, Dr. Reynolds says, "these cases are, so far as my observation has extended, much less numerous than the other two."

The rest of the paper is occupied with some considerations of a speculative character, which possess no novelty, and with some remarks on treatment.

#### ART. 39.—*On the use of Cotyledon Umbilicus in Epilepsy.*

By DR. PEACOCK, Assistant-Physician to St. Thomas's Hospital.

(*Medical Times and Gazette*, Aug. 11, 1855.)

In this journal we find the particulars of four cases of epilepsy treated with this remedial agent without any beneficial results. The extract was of unquestionable excellence, and the usual dose was from three to six grains daily. In all the cases, the greatest care was taken to detect any effects which might result from the use of the medicine, and in none were any symptoms observed which could be referred to its action, though, in the last two cases, the doses employed were very large, and the medicine was tried for several weeks. In one case, the increase of the dose corresponded with a temporary improvement in the patient, he being for three weeks without any fit, though generally they had occurred in greater or less number every week; but a perseverance in the use of the medicine showed this improvement was only coincident, the fits again recurring as frequently and as severely as before; and it was further ascertained that similar cessations had previously been observed for even a longer period. In the three cases, which had before been treated with the sulphate of zinc, the patients and their friends all considered that they were better while under that treatment than when the cotyledon was exhibited.



ART. 40.—*The use of Belladonna in Epilepsy.*

By Professor TROUSSEAU.

*(Medical Times and Gazette, Aug. 25, 1855.)*

M. Debreyne, physician to La Trappe, and M. Bretonneau, undertook, twenty-five years since, in two different parts of France, a series of patient researches into the results furnished by belladonna, already recommended by Storck; and founding their opinion upon some cases which seemed conclusive, proclaimed the superiority of this therapeutical agent; unfortunately, such superiority is only relative. For twelve years I have employed it, having always had under treatment from eight to ten persons. In some of these patients the belladonna has completely failed, in others it has produced some melioration, while in some cases—these being, it is true, the smallest number—the greatest advantage has been derived from its use. I have so treated 150 patients, and of this number twenty have been cured, if they do not even yet relapse; and M. Blache has employed it during the same period in his large private practice, with a like proportion of successes and failures.

The mode of administration plays a great part in this medication, at which we need feel no surprise, as this is the case with the most powerful specifics. Thus Torti declared, with justice, that a pound of bark, administered without method, would not cut short a fever that two ounces, properly given, would cure. Pills are to be formed, composed of extract and of powder of the roots of belladonna  $\text{āā } \frac{1}{7}$  grain. A pill is to be given every night for a month, and two pills every night during the second month. For the third month, three pills, and for the fourth month four pills, are required; the entire number, whatever this may be, always being taken as one dose. If we find the patient is very susceptible to the action of belladonna, we must only increase the dose every sixtieth day. During all this time the family must keep a register, in which are entered the number and nature of the fits or vertigos; and if by the end of a year you have obtained a sensible diminution in the number and duration of attacks, you may reckon with certainty on the cure, providing the medicine be continued from two to four years, the dose not being increased after the physiological action of the drug is sufficiently manifested. We must not be surprised at this lengthened period of treatment, for such is necessary in almost all chronic affections. Before ceasing its administration entirely, and especially if it is borne with difficulty, it may be suspended for two or three, and then for four months, resuming it for a month, and in a diminished dose.

ART. 41.—*Case of Traumatic Tetanus.* By Mr. GEORGE GARNHAM.*(Lancet, Aug. 18, 1855.)*

This case is related, and three others are alluded to, for the purpose of showing that the amelioration in the tetanic symptoms was concurrent with the appearance of mercurial action. “Although in one

case bleeding was used, in another digitalis, in a third turpentine dressing, and in a fourth chloroform (topically), yet the four cases were all treated with calomel and opium, and the first sign of the diminution of the disease was contemporary with the tenderness and swelling of the gums." So speaks Mr. Garnham. Now all this we can understand, for we hold that all spasm is absolutely incompatible with any kind of inflammation.

CASE.—G. G——, æt. 27, a strong man, of sanguine temperament, on the 23d of August, 1854, while riding, was thrown from his horse, and received a contused wound on the knee, just over the superior edge of the patella. The wound appeared little more than a deep graze; but after the lapse of a few days, during which poultices were applied, great swelling having taken place on each side of the knee, a slough separated, disclosing a deep but healthy-looking ulcer. On examination, the integument was found to be separated from the parts beneath to the extent of three inches, on either side of the knee; and, by pressure, a great accumulation of healthy-looking pus was discharged. Nothing worthy of notice occurred up to the evening of the 9th of September, when my attention was directed to a slight stiffness in the action of the jaw; the ulcer, too, which had up to this time been covered with healthy pus, presented a dry appearance, the edges being of a dark red, approaching to livid. He complained of a slight cold, which he attributed to his having approached the open window without his coat. The bowels being costive, I ordered him calomel, two grains; compound extract of colocynth, eight grains; immediately: to be followed three hours afterwards by a black draught. The jaw to be fomented, and rubbed with a liniment, composed of liquor of opium, two drachms; and compound soap liniment, ten drachms. The bowels were freely opened from the purge; but the stiffness of the jaw continued to increase almost imperceptibly till the night of the 14th, when symptoms of acute tetanus set in, spasms occurring at intervals of very few minutes. Early in the morning of the 15th, I was sent for, and procured the assistance of Sir John Fife. We dressed the wound with chloroform, covered by a poultice; and prescribed a febrifuge mixture, with twenty minims of tincture of digitalis at a dose, every three hours: a blister between the seat of injury and the spinal cord: and pills, containing calomel, four grains; and opium, one grain and a half: to be taken every three hours. The jaw being now constantly almost closed, we ordered friction every three hours, with a liniment of equal parts of compound soap liniment and tincture of aconite. The pulse, which had hitherto never exceeded 90, during the night of the 14th reached 120.

Sept. 16th.—He had passed a very restless night, sleep being entirely prevented by the frequency of the spasms; pulse 100; bowels slightly relieved, stool liquid, very dark-coloured, and offensive; spasms not so frequent as during the night; complains of tension of the abdomen, causing vomiting. The muscles of the face towards evening were much relaxed; mouth open to the extent of three-quarters of an inch; can drink easily.—10 p.m.: Symptoms all somewhat increased.—12 p.m.: Slightly delirious; spasms violent every four or five minutes.

17th, 4 p.m.—Pulse 108; sleeps half an hour at a time between the spasms; complains, in passing urine, of the stream stopping suddenly, apparently from spasm.—6 a.m.: Pain in the bowels, and is sick eight or nine times within half an hour; the breath has slight mercurial fætor. Reduced the dose of calomel to one grain and a half.—8 p.m.: Pulse 96; spasms of abdominal

muscles seem somewhat increased; purged, stools slightly tinged with blood.

18th.—Pain in the bowels, purging, and bloody stools, still continue; pulse 104, very weak; mercurial action fully established; severe spasms only precede evacuations from the bowels; the wound suppurating freely. Applied another blister in the same line with the former one, to be dressed with savine ointment: ordered the calomel to be discontinued, and one grain of muriate of morphia at night: and a mixture composed of liquor of acetate of ammonia, two ounces; tincture of digitalis, two drachms; water, six ounces; one ounce every three hours; and a pill every three hours also, composed of one grain of opium, and three grains of carbonate of ammonia. He has drunk half a bottle of sherry in the course of the day, and also a good deal of beef-tea.

20th.—Spasms during the day rather frequent, but not so severe, and principally affecting the lower extremities; pulse from 100 to 140.

25th.—Since last date the spasms have been gradually decreasing in force and frequency, and he is now able to stop the spasm of the leg, by grasping tightly the superior part of the thigh.

From this time no bad symptom made its appearance; the wound healed easily, and he is at this moment in the enjoyment of his usual health.

## (B) CONCERNING THE RESPIRATORY SYSTEM.

### ART. 42.—*On the pathology of Hooping-cough.*

By Dr. GRAILY HEWITT, Surgical Registrar to St. Mary's Hospital.

(Pamphlet, Churchill, 1855.)

This short essay will well repay perusal. Its object is not so much to discuss the essential nature of the disease, as to set forth its effects upon the lungs, and to show that these are other than the consequences of pneumonia. The essay is based upon nineteen carefully recorded cases.

It is very well known to those who are acquainted with the literature of the subject that the collapsed condition of the lung, which is so often met with in hooping-cough, and generally designated under the term "lobular pneumonia," is not pneumonia in the ordinary sense of the word. Dr. Alderson, who was the first to direct attention to this important fact, called this condition "carnification." Subsequently carnification was shown to be identical with the condition of the lung which is met with in new-born children—the *atelectasis* of Jorg. This opinion was stated by Legendre and Bailly, in 1844, in their valuable treatise on the pulmonary affections of children.

"The portions of lung affected with the so-called lobular pneumonia were found by them to be inflatable, the effect of the inflation being to restore to the portions affected their normal appearance and qualities. For the name "lobular pneumonia" they substituted the term "*état fœtal*." They pointed out the difference which existed between the effects of inflation on the portions which exhibited the fatal conditions, and on those affected with true pneumonia, the inflation producing no effect on the latter, while it restored the former to their normal appearance and qualities. The anatomical character of this "fœtal condition" they described as follows:—The portions of



lung affected were depressed below the surface of the adjacent healthy lung, non-crepitant, firm, compact, sinking in water, section showing cellular interspaces, the colour of a red violet, sometimes darker, consistence variable, sometimes friable, the section smooth and not granular. To these characters was added the important one of the complete inflatability of the affected portions. Two varieties of this foetal condition are described—the one simple, the other congested, the names sufficiently indicating the distinctions between them.”

This opinion has been abundantly verified by subsequent experience, but the fact is not so well known as it ought to be, and therefore we are glad that Dr. Hewitt has come forward to elucidate it. The name which is now generally used to express this condition of lung is *apneumotosis*. This was given it by Fuchs, and it is perhaps more expressive than any other.

Dr. Hewitt's observations, as we have said already, were nineteen in number.

“The ages of the children who were the subject of them varied from four years to one month, the average being eighteen months. In all, the state of the lungs was carefully noted. The chief lesion found after death was collapse of the lung substance. The following is a statement of the degree to which this pathological condition manifested itself in the different lobes of the two lungs.

“In the *right lung*, portions of the upper lobe were found collapsed in six cases, and in four more to a less degree.

“The middle lobe was collapsed, wholly or in part, in sixteen cases.

“The lower lobe was more or less affected with collapse in eighteen cases.

“In the *left lung*, the upper lobe presented the same lesion in fifteen cases, the whole of the anterior tongue-like prolongation being in most of the cases affected.

“The lower lobe was collapsed more or less in eighteen cases.

“In seven of the cases, the portions collapsed were also congested—in some to a high degree.

“The test of MM. Bailly and Legendre, viz., the inflatability of the portions of the lungs thus affected, was used in almost all the cases; and on that and other grounds it was determined, that the particular part of the lung in question was collapsed and not hepatized.

“It will be at once perceived, that the occurrence of collapse was almost universal; all the cases, with the exception of one, in which there was extensive tuberculization of the lungs, presenting a greater or less amount of lung substance affected in this manner.

“The collapsed portions were found to have the following general characteristics. They were abruptly separated from the adjoining healthy lobules, depressed below the general surface of the lung, less bulky than the unaffected portions. The colour varied from a reddish violet to a deep purple; the firmness was variable, in most cases, however, having a great resemblance to that of a piece of flesh, non-crepitant, sinking immediately in water, lobular cellular interspaces well marked. No air-cells visible in the surface, or on section, even with the aid of a lens. Section of collapsed portions showed a uniform smooth surface, slightly friable in some cases, and emitting on squeezing

a small quantity of non-aërated puriform fluid. The lung substance did not break down under pressure, as is seen in hepatization. When a blowpipe was introduced into the bronchus leading to the affected portions, and inflation performed, the aspect of the collapsed portions underwent a striking change. They immediately assumed the appearance of the adjacent healthy lobules, and were in no wise to be distinguished from them—becoming enlarged, and the air-cells on the surface easily distinguishable by the aid of a lens. The colour was changed from a dark violet to a light pinkish hue, such as is habitually seen in the healthy lungs of children. The lung substance was found then to float readily on water, and to have become crepitant. When these inflated portions were left to themselves for a short time, they became to a certain degree collapsed; the lung contracting and expelling a portion of the air artificially introduced. The inflation was performed with ease in most of the cases; in some, however, the force necessary to be used was more considerable, and some portions were not inflated at all by the additional force used. The portions which occasionally resisted full inflation were the posterior surfaces of the lower lobes.

“The depth to which the lung substance was implicated was variable. In all cases the collapse exhibited a preference for the portions of the lobes most distant from the root of the lung—thus the margins of the lobes were found chiefly affected. A great part of a whole lobe was, in many cases, collapsed deeply as well as superficially; the upper lobes, however, were never found very deeply affected.

“The anterior tongue-like prolongations of the two upper lobes were, in nearly all the cases, collapsed, and were thin, pliable, and *lobulated*, to the feel, if I may be allowed the use of such a term. The external surface of the upper lobes often presented little digital pits or depressions, the depressed surfaces being of a colour approximating to violet, and constituted by lobules in a semi-collapsed state. Inflation quickly gave the lobe a uniform, smooth surface.

“Such was the general appearance and character presented by the collapsed portions. In many of the cases these portions were themselves the seat of other alterations, to which I shall now allude. The collapsed portions, in several instances, were spotted on the external surface, which was due to the fact that certain air-cells, either singly or in groups, were distended with a muco-puriform fluid. They were chiefly seen on the external surface, but a section also showed them, though less distinctly. The patches thus constituted were of a variable size, but mostly as large as a millet-seed, very slightly elevated above the surface, of an opaline grey or yellowish colour. On pricking them with the point of a lancet, a small quantity of puriform fluid exuded, and the little eminence disappeared. They were very different in appearance and general characters from tubercular deposits, for which, however, they might, at first sight, have been taken. They were identical with what has been described by Legendre and Bailly as the first and second stages of their catarrhal pneumonia. Section of the lobules affected in this manner exhibited similar spots or patches. A further stage of this process was exhibited in some of the cases, where cavities of a larger size were found occupying the terminal extremities

of the bronchial tubes. They were, for the most part, scattered, and not very numerous, always situated in portions of the lung which were collapsed."

True hepatization of the parenchyma of the lung was only met with in four out of these nineteen cases, and in these it was very partial. In all, there were signs of inflammation in the bronchial tubes, especially in the smaller ones; and in all, the non-collapsed portions of the lung were more or less emphysematous, with here and there large vesicular dilations. As to the rest there was nothing remarkable.

We have only one comment to make upon these facts. It is very clearly shown that pneumonia, properly so called, has little to do with whooping-cough, and we are left to suppose that bronchitic inflammation plays some important part. Is it so? Has not the inflammation subsided, in those cases where it ever existed, before the supervention of the spasmodic phase of the disorder, and are not those the traces of congestion, not of inflammation, which are written upon the bronchial air-passages at this time? The general symptoms, we think, are quite conclusive upon this point, and we cannot find any contrary evidence in the condition of the lungs after death. It is to congestion, and not to inflammation, that the condition of the lungs points.

Dr. Hewitt adopts Dr. Gardner's views in accounting for the collapsed and expanded condition of these cases, and he explains these views. He also discusses some other topics. He considers a shallow and imperfect, and therefore very quick respiration—sometime as high as 75 or 80 in a minute, without any marked indication of distress, such as are met with when the breathing is very quick in acute affections of the lungs,—as one of the symptoms of the collapsed condition of the lungs; but upon this and other points we must refer to the essay itself, merely repeating, what we said at the beginning, that it will well repay perusal.

ART. 43.—*On the contagiousness of Whooping-cough.*

By PROFESSOR TROUSSEAU.

(*Medical Times and Gazette*, June 2, 1855.)

The following remarks occur in a clinical lecture recently delivered in the Hôtel Dieu at Paris:

"We have had a slight epidemic of pertussis in the children's wards, where it was introduced by a single case. During a certain period, all the children in the ward, and those who were admitted, acquired the disease. Then, at a certain period, although there were from eight to ten children at the height of the disease, others were admitted without any of them catching it. Thus to render contagion possible, certain conditions are necessary, which are no less real for being undetermined."



ART. 44.—*The use of Quinine in Hooping-cough.* By M. LECADRE.

(*Journ. de Med. et Chir. Prat.*, July, 1855; and *Dublin Med. Press*, July 18, 1855.)

"We seldom see hooping-cough declare itself at once, it is almost invariably preceded by an *incubatory* bronchitis with fever. When this bronchitis appears in a child during the prevalence of an epidemic of hooping-cough, we may suspect the existence of the latter, but it is not yet present with its fits and nervous symptoms; the period has not arrived for administering the sulphate of quina. This is especially the time to employ emetics, particularly ipecacuanha, emollients, and revulsives to the extremities. The salt of quina should be reserved for the second or spasmodic stage. It is then exhibited in powder at the rate of from three grains to half a scruple daily, according to the age of the child, care being taken to administer the dose immediately after the fit, in order to give the remedy time to act before the next paroxysm.

"When the doses are regularly taken, it is unusual not to observe a sensible modification in the attacks. They are shorter, and consequently more tolerable. After two or three days' employment of the remedy, the paroxysms become less frequent. The disease does not entirely disappear; we know that unfortunately it has almost always a fixed duration of from forty to fifty days, but at least it is supportable, the fits occur at longer intervals, and are free from any alarming character.

"Before I thought of trying sulphate of quina in the treatment of hooping-cough, I had employed all the means recommended in that disease; belladonna in all forms, assafoetida, sulphuret of potassium, coffee, cochineal, &c. None of these remedies seemed to possess the sedative power of the sulphate of quina. At first I even employed some of them concurrently with this salt, but I thought I derived more advantage from its use when I gave it alone, and I have therefore latterly confined myself to it. I at most prescribe, in addition, a pectoral infusion, and sometimes a spoonful of syrup of lactucarium when the little patient is deprived of sleep."

ART. 45.—*The insufflation of powdered Nitrate of Silver in Laryngitis.*  
By M. EYFERT.

(*Annalen des Charité Krankenhauses*, Bd. v; and *Med.-Chir. Rev.*, July, 1855.)

Trousseau was the first to recommend the inhalation of caustic powder as a simple and effectual mode of treatment in certain forms of laryngitis; and since this time several persons, and now M. Eyfert, have borne testimony to the same effect. This plan is much more easily carried out, and quite as effectually, as the analogous plan of applying caustic solutions to the larynx, and M. Eyfert directs particular attention to this point. The powder inhaled was composed of three grains of nitrate of silver in a drachm of sugar of milk, and about as much as would lie upon the barrel of a steel pen was inhaled daily.

The inhalation is conducted in the following manner:—A steel pen, charged with as much powder as it will hold, is attached to one end of the barrel of a quill, which is also open at the other end. This is introduced far enough into the mouth to bring the steel pen opposite the root of the tongue. The lips are now closed around the quill, and the nostrils compressed, while the patient is desired to draw in air rapidly and forcibly through the quill barrel. Almost every one fails at first, but all succeed on the second or third attempt—the cough and irritation of the larynx announcing the penetration of the powder there. Even delicate females and children easily practise the insufflation, and will repeat it for days or weeks together. Young children may have it administered by an apparatus contrived by Professor Burow. M. Ebert has as yet only employed the remedy in laryngitis; and he briefly relates six cases of its successful application.

ART. 46.—*On Rheumatic Pneumonia.* By M. TROUSSEAU.

(*Medical Times and Gazette*, June 16, 1855.)

M. Trousseau is speaking in a clinical lecture:

“About a month since, a young man was admitted with all the signs of pneumonia, and, kermes having been administered, the next day all traces of pneumonia had disappeared. To what were we to attribute so sudden a retrocession? Was it the result of treatment, or must we seek for the cause in some peculiarities attaching to the nature of the disease itself? The latter interpretation received some light from what was observed at the next visit, when the left great toe was found red, swollen, and painful, the tendinous sheaths along the dorsum of the foot exhibiting a like condition. Next day, the right foot was similarly affected, though in a less degree. Two days ago, a woman was admitted with the following symptoms: strong febrile action, redness and swelling of the left leg and foot, and severe pain in the entire upper extremity and trunk of the same side, the pain exciting cries on moving the parts. The patient especially suffered at the left side of the chest, but no abnormal sounds were audible. During the night cough came on, and in the morning a manifest *souffle* was audible in the supra-spinal fossa, while around and in the infra-spinal fossa was heard a fine sub-crepitant *râle*. During the cough, dry crepitating *râles* and bronchophony were heard, and two or three pneumonic sputa were expelled. This morning all signs of pneumonia have vanished. Here again I hesitate to attribute such prompt resolution to the treatment, especially as the apex was the part involved—a form of pneumonia regarded by all physicians as especially serious. I prefer explaining so rapid a termination by the nature of the pneumonia itself, which I regard as *rheumatic*.

“Too partial to localization, practitioners are only accustomed to recognise rheumatism as affecting certain tissues, viz., the muscles, the aponeuroses, and the joints, and when it manifests itself elsewhere they call it by some other name. This is as if we only acknowledged syphilis as we observe it on the penis, and made so many distinct affections of its manifestations on the throat, skin, &c. But syphilis

is recognised to be the disease in all these accidents, and why should it not be so with rheumatism? That it attacks all serous membranes is an indisputable fact since Bouillaud's beautiful researches, which have so much advanced the pathology of the heart. When in the course of acute articular rheumatism any of the serous membranes become affected, it is termed a pericarditis, pleuritis, meningitis, &c., according to the membrane attacked. This is right enough as far as it goes; but for the proper denomination of the disease, which is a kind of definition in a single word, we ought to add the epithet "rheumatic." When a man accustomed to suffer from rheumatism acquires, as a consequence of cold, a pain of the shoulder, hip, &c., he at once says he has an attack of rheumatism. But instead of this pain let there be a sore throat, and both patient and doctor cease to be logical, and call it angina instead of rheumatism; just as if there were not a true rheumatic pain of the fibrous parts of the pharynx and palate, pain followed by fluxion, tumefaction, and redness of the pharyngeal mucous membrane. Do we not find rheumatism of the fibro-serous tissues of a joint accompanied by tumefaction of the subcutaneous cellular tissue, and bright redness of the skin; and why should we not admit the same influence in the delicate and vascular mucous membrane? For my part I should not hesitate to recognise a rheumatism in such a case, or, if you like it better, a rheumatic angina.

"This distinction may serve for the explanation of the very great differences observed in the progress and termination of anginas, regarded by some physicians as being of the same nature. Thus, simple inflammation of the tonsils goes through all its stages, in spite of whatever treatment may be opposed to it, and a patient accustomed to such attacks will warn his attendants of the inutility of endeavouring to prevent the formation of abscess. A rheumatic angina, on the contrary, will often disappear in the course of a night, whatever the treatment adopted, leaving the physician astonished at his therapeutical success, the result, however, being really due to the essentially mobile character of the affection. Descending lower down in the digestive canal, we can explain those sudden diarrhoeas which manifest themselves under the influence of a chill. The fibrous portions of the canal become painful, and the contractions more considerable and more frequent, a fluxionary movement being at the same time established towards the mucous membrane, the secretions of which are increased. Such diarrhoeas are of short duration, unless, indeed, the rheumatism take on, as it may anywhere, a chronic character.

"After these considerations, does it seem strange to admit a rheumatic pneumonia? Suppose the pulmonary tissue, or, what is the same thing, the fibrous tissue of the extremity of the bronchi, becomes seized with rheumatism, what are the immediate results? Tumefaction and congestion of the mucous membrane, and an infiltration of the cellular tissue; that is to say, the anatomo-pathological conditions of œdema or of pneumonia in its earliest stage; with this peculiarity, that such lesions, participating in the fugacious nature of rheumatism, do not possess the fixity and persistence of the lesions of ordinary pneumonia. It is in cases like these that therapeutical results seem so marvellous, and so they would in our own two cases had we not a



better reason to give for the rapidity of the cure. They were, in fact, the examples of rheumatic pneumonia, the one occurring in a young man who was at the same time suffering from rheumatism of both feet, and the other in a girl who had formerly had rheumatism, and together with the pain in the chest complained of rheumatic pains along the whole of the same side of the body. In similar cases, I shall not hesitate to admit the existence of rheumatic pneumonia, too happy only thus to complete my diagnosis, and to become enlightened as to the amount of importance that should properly attach to my therapeutics."

ART. 47.—*Case of Pneumonia, with solid casts in the bronchial tubes.*  
By Dr. WILKS.

(*Pathological Transactions*, vol. vi, 1855.)

J. J., æt. 47, was admitted into Guy's Hospital on March 8th, 1855. He was a mason, and said he had been ill seven days, and was understood to state that he had walked from his lodging, at Limehouse. When he went to his ward, he said that he felt better, and would not go to bed, but sat up and took his tea. Shortly afterwards Mr. Stocker, the apothecary, saw him, ordered him to bed, and examined his chest. That gentleman found the right side universally dull on percussion, and a total absence of all sound during respiration. The patient died on the following morning.

On a post-mortem inspection, the whole right lung, except at its very lowest part, was in a state of grey hepatization, and the bronchial tubes were filled with solid casts of lymph. These penetrated as far as could be dissected, and, no doubt, were connected with the exudation in the air-cells. All the casts joined in the right bronchus. The tubes in the very lowest portion of the lung, which was comparatively healthy, were quite free of contents. The casts were composed of the ordinary material found in an acutely inflamed lung, a delicately fibrillated substance, in which were interspersed a number of granular exudation-cells. The tubes which contained them were perfectly healthy; the mucous membrane showing no traces of inflammation, and not the slightest adhesion existed between the walls and the contents.

The specimen is an example of an exceptional occurrence in pneumonia, but one which occasionally is met with. What its pathological nature or peculiarity is, requires further elucidation. Unless a very accurate statistical account were kept, in which the tubes had been examined in all cases of pneumonia, no positive answer could be given to the question as to the frequency of its occurrence, but probably it would be only met with once in every thirty or forty cases. Is this filling of the tubes with solid lymph accidental, dependent upon a peculiar condition of the mucous membrane of the tubes themselves, or a mere excess of the inflammatory process, or has it a peculiar pathological significance? It is tolerably certain from the healthy condition of the bronchial membrane, that the exudation does not occur from it, and therefore that the affection is in no way

allied to that of plastic bronchitis; but that either from a superabundance of inflammatory material, or from the rapidity with which it is poured forth, the material thrown out in the air-cells passes quickly into the tubes, and there coagulates. Dr. Wilks, in a large number of dissections of pneumonic lungs at Guy's Hospital, had only met with five instances, of which the present was the last,—one had occurred during the same month, another two years before, and the others were anterior to this. In the first two cases the patients were otherwise diseased, having both of them granular kidneys. The other cases had occurred during a period when pneumonia of a low form had been rife, and when an epidemic influence had been believed by many to exist. From these few examples, then, the disease would be looked upon as one of an asthenic character, due either to an individual peculiarity of the patient himself, or to an external atmospheric influence. Such an opinion is not contra-indicated by our knowledge of other cases where there is a tendency to the deposit of fibrin or solid material from the blood. How great this disposition was in the present instance may be shown from the fact that the difference in weight between the two lungs amounts to four pounds and a half. Thus this large amount of material, of which a great part was solid, had been delivered up by the blood within the short space of seven days.

A very important practical point connected with this complete filling of the tubes, is the total absence of bronchial sounds which necessarily results from that condition.

ART. 48.—*The Cracked-pot Sound not always indicative of a cavern.*

By Dr. J. HUGHES BENNETT.

(*Edinburgh Monthly Journal*, Feb., 1855.)

Dr. Bennett speaks as follows in a clinical lecture upon a patient named M'Kay :

"According to Skoda, 'the cracked-pot sound is heard in the thorax, over tolerably large and superficially situated cavities which contain air, and communicate with the bronchial tubes. When the percussion is forcible, or the thoracic walls flexible, the cavity is compressed at each stroke, and a portion of air suddenly driven out of it into the bronchial tubes: this hissing murmur, caused by the escaping air, is mixed up with the ordinary percussion sound of cavities, and this compound represents the cracked-pot sound.' But the observation made in the case of M'Kay, has satisfied me that occasionally distinct cracked-pot sound may be elicited over condensed lung, without any cavity whatever. In referring to an excellent paper on this subject by Dr. Markham, I find that on one occasion both he and Dr. Sibson have noticed this phenomenon, over the upper portion of a lung which was afterward shown to be gorged with blood and serum, though still retaining some portion of air. It would seem, from what has been said by Skoda, Stokes, Walshe, as well as by Dr. Markham, that a peculiar tympanitic sound may be heard over collapsed or condensed lungs, when covered or mingled with a certain amount of air. For instance,

when in cases of pleuritic exudation, air is effused into the pleura, a few hours before death, when in certain cases of pneumonia there is also emphysema, &c. In the case I have alluded to, these conditions were so far fulfilled, that the tubercular mass described was surrounded by spongy lung full of air. At all events, it must be evident that the physical conditions on which this peculiar sound depends, require more careful study, and that our ideas as to its necessary connection with a cavity, must undergo modification."

ART. 49.—*On the transmission of sounds from the root of the Bronchi to distant parts of the Thorax.* By M. BARTHEZ.

(*L'Union Médicale*, No. 67, 1855; and *Medical Times and Gazette*, Aug. 25, 1855.)

Although in the majority of cases an abnormal sound may be said to indicate lesion of the part of the lung placed under the ear that perceives it, there are some cases in which such sound is transmitted from a more distant point. It is the object of M. Barthez' memoir to indicate some of the anatomical conditions favorable to such transmission, so as to guard against the diagnostical and therapeutical errors that may arise from ignorance of their existence. He refers to a case in which phthisis was declared to exist, inasmuch as repeated examination had detected cavernous respiration and gargouillement at the apex; but after death no cavity could be found, but only a pleuritic effusion that thoracentesis might perhaps have relieved.

When a solid body replaces a portion of the lung, it may become the conductor of sounds produced in the trachea or the bronchi, providing that it be in contact with the point of the chest opposite to the ear, and also in immediate contact with the large air-tubes. Tubercular bronchial glands, united or not to tubercle or other induration of the lungs, are the most common of these causes, as insisted upon already by the author, and M. Rilliet, in their great work on the 'Diseases of Children.' Another point advanced in the same work is that fluids effused into the pleura may become the means of such transmission. Bronchial respiration is then very frequent, while cavernous respiration, gargouillement, and amphoric respiration are by no means rare, and may deceive the most experienced stethoscopists. As the result of more matured experience, he now wishes to show:—1. That the presence of fluid in the pleura influences these symptoms; their existence and seat varying according to its amount. 2. That the presence of a co-existent solid body is favorable, and perhaps indispensable, to the transmission of the sounds. 3. That nevertheless the sounds so perceived are conducted by the fluid, which may even exaggerate them and impart to them a special *timbre*.

In all the cases in which the author has made an autopsy, there has been co-existence of effusion and a solid body, sometimes a chronic induration of the lung, maintained by partial adhesions, sometimes a pneumonia, at others tubercles or glands united to a mass of tubercular lung, and finally aneurism of the aorta. He is disposed to believe that the sole effect of the foreign body is, usually, the establishment of a continuity of vibrating tissue between the air-tubes and



the fluid. Thus, 1. When solid bodies alone transmit tracheal sounds the phenomenon is perceived only at the root of the bronchi, in the supra-spinal fossa, and rarely below the clavicle, *i. e.*, over a restricted portion of the chest. When there is pleurisy with effusion, on the other hand, the tracheal sounds may be heard over the whole of the upper part of the chest, frequently in the infra-spinal fossa, and sometimes at the base of the thorax, almost always over a considerable space. 2. When the fluid is absorbed, we may often perceive the rubbing sound at the point where the cavernous or amphoric respiration was at first audible. 3. The autopsy exhibits the fluid at the level of the points where the transmitted sounds were audible, and sometimes where they manifested the greatest intensity. 4. The passage of sonorous waves through the liquid may exaggerate them and modify their *timbre*. A solid body alone never induces that variety of respiratory sounds which resembles the amphoric respiration. But, whatever name may be given to these pleuritic sounds according to their intensity, or rather according as they simulate bronchial, cavernous, or amphoric respiration, they possess a special *timbre*, which a little habit prevents our confounding with sounds produced immediately under the ear. The difference is analogous to that prevailing between bronchophony and œgophony; and for all such sounds M. Barthez propose the term *hydric*, as indicative of their having traversed a liquid. It is in auscultation what the special sound indicated by Skoda (and which often exists at the apex of the chest in pleurisy) is in percussion. This comparison between these two sounds is quite justified by their frequent coincidence. In a considerable number of patients the two have arisen, progressed, and disappeared, almost simultaneously, and, therefore, they probably depend upon the same anatomical conditions. The transmission of the sounds is easiest in a small and narrow thorax, with thin and dry parietes, and during exaggerated respiration.

#### (C) CONCERNING THE CIRCULATORY SYSTEM.

ART. 50.—*Paracentesis of the Pericardium.* By M. JOBERT.

(*Gaz. des Hôpitaux*, Feb. 8, 1855.)

This case occurred in the wards of M. Trousseau, in the Hôtel Dieu, and the result was successful.

CASE.—The subject was a young man *æt.* 16; pale, debilitated, suffering with intense dyspnœa, and considerable dulness in the precordial region, which extended from the second rib above and to the right of the sternum, being six and a half inches in length by seven in width, with marked prominence of the left side of the chest. Under the use of digitalis and blisters the effusion continued increasing till the dulness reached the clavicle, the patient becoming daily more emaciated and feeble. As death appeared imminent, puncture of the pericardium was determined on. An incision was made in the fifth intercostal space, an inch from the left border of the sternum, involving the skin and cellular tissue. A trocar was plunged obliquely from within outwards across the intercostal muscles, and was made to penetrate slowly and by a continued movement into the cavity, when the stem was

withdrawn and from the canula escaped a little brown serum. The canula left in the wound was agitated by the pulsations of the heart, and raised by each contraction. The canula was left in for one hour and a half, and thirteen ounces of serum escaped. The distressing symptoms disappeared after the operation; the respiration was quiet; pulse good, full one hundred and thirty-four; dulness diminished three inches below the clavicle. The improvement progressed for some days, when an effusion in the left pleura was found rapidly increasing, which became so grave as to require an operation for its removal. The trocar was first plunged in the intercostal space on a level with the axilla, but meeting with a very resistant false membrane no fluid escaped. A second puncture, made a little lower down and more posteriorly, evacuated a pint of fluid. The operation was not followed by any accident. Neither the effusion into the pleura nor pericardium had been reproduced when the patient left the hospital, one month after the operation.

ART. 51.—*Affection of the Heart, Thyroid Gland, and Eyeballs.*

By (1) MM. ROMBERG and HENOCH, and (2) Dr. JOHN T. BANKS.

1. (*Klin. Wahrnehm. u. Beobachtungen*; and *Edin. Mon. and Surg. Journ.*, April, 1855.)
2. (*Dublin Hospital Gazette*, June 1, 1855.)

(1) This disease, to which attention has been drawn by Marsh, Begbie, Cooper, &c., in Great Britain, seems also to be well known in Germany, and many examples of it have been observed by Pauli, Brueck, Basedow, and lastly by the authors whose interesting paper we have now before us—Romberg and Henoch. Though differing in regard to the etiology of the disease as a whole, and disagreeing to a certain extent in the account given of the rise and occasion of its individual symptoms; still, in the descriptions of all the writers now named, there exists so remarkable a uniformity, as to satisfy us of the identity of the disease which each has observed.

We shall, in the first place, make our readers acquainted with some of the cases in an abridged form, and the remarks of Romberg and Henoch, and then add a few observations of our own, which the perusal of the former have called forth.

CASE 1.—A. S., æt. 14, who had never menstruated, was treated, in the clinical ward, for anæmia, and cured by a preparation of iron. In October, 1849, she again became a patient, her former disease having returned. At that time, the extraordinary paleness of her skin revealed her anæmic condition. The right lobe of the thyroid was swollen, and the jugular vessels were seen pulsating. The anæmic sound was clearly audible in the neck. There was the evidence of an enlargement of the heart, and its first sound was at the base accompanied by a bellows murmur. The patient suffered from dyspnœa, increased by motion and from great weariness. The bowels were irregular. On the 12th November she was ordered to take iron, which, with a short interruption, she continued to do till January, 1850. At that date, a decided improvement in her whole system was visible.

CASE 2.—A girl, æt. 18, who had first menstruated a year previously, began to complain of violent palpitation and uneasiness in the region of the heart, brought on chiefly by exertion, especially in ascending stairs. At the same time, a swelling had appeared in the front of the neck, and at times she expectorated blood. The diagnosis, after examination of the heart, was that

something more than functional disorder existed, and that valvular disease was present. In this patient, as in the former one, the enlargement of the thyroid and the affection of the heart existed, but the eyes were not implicated. She differed from the former case in not presenting an anæmic appearance.

CASE 3.—Mrs. R., æt. 47, subject for many years to numerous hysterical complaints, was admitted into the Clinic, May 18th, 1849. She complained especially of violent palpitation of the heart, and consequent agitation. During the attacks of palpitation she experienced a feeling of tightness in the throat, and a glimmering before the eyes. The thyroid was evidently enlarged, particularly in its right lobe, and in it she experienced a sense of pulsation and of pain during the occurrence of the palpitation. Then, also, the eyes became unusually large, and appeared starting from their sockets in such a manner as to expose her to the laughter of bystanders. The catamenia were regular but scanty. The pulsations of the heart were increased to 100 in the minute, but otherwise there was no change detected. She was hysterical, suffered from weariness and from irritability of temper, with inclination to weep. Although the symptoms had existed for nearly two years, they had considerably increased during the previous four months. In this patient the manifestation of anæmia was very clear. A mixture of digitalis, with phosphoric acid and valerian, were the remedial means employed, and amendment was so speedy that in July of the same year she was able to take a situation as lady's maid, and went to Dobberan, where she experienced great benefit from the use of the sea baths. In March, 1850, she was seen entirely freed from her former ailments.

CASE 4.—C. L., æt. 20, first menstruated at the age of thirteen, and suffered thereafter from complete amenorrhœa for a whole year. During this period she had a trifling swelling on the front of the neck. Menstruation again returned, but very irregularly, and the girl, from being blooming and robust, became pale and weak; the thyroid swelling increased; palpitation of the heart succeeded, and then followed a strange largeness of the eyes. Considerable benefit was obtained in this case from the steady use of iron.

CASE 5.—Mrs. B., æt. 25, presented herself at the Clinic on July 7th, 1848. Always healthy, and having regularly menstruated. She had for three weeks been occasionally exposed to a draught, when washing, with her neck uncovered. She had felt pain in the front of the neck, and had latterly noticed a slight swelling in the situation of the thyroid. On examination at the Clinic, the same remarkable prominence of the eyes as noticed in the other cases was observed, still the sight was not affected. Violent action of the heart existed; and in the thyroïdal tumour, now greatly increased, and very large, as also in the head, the pulsation was inordinate. The pulse was 144. Three cups of blood were taken, and in the following week leeches were applied to the enlarged thyroid; from these measures an evident, though temporary, amelioration followed. Afterwards (early in 1850), from the use of digitalis, and due in great measure to the regular return of the menses, which had been much interrupted, restoration to sound health occurred.

CASE 6.—A. B., a young girl, æt. 17, had suffered from violent palpitations for two years; had also an attack of typhus fever, and had been neglected, owing to living in a country village. Was admitted into the Clinic, November, 1847, when her appearance was most striking. The eyes were protruded; the sight, however, unaffected. The thyroid was greatly enlarged, and appeared throbbing; a loud systolic murmur was heard, and peculiar thrill felt over it. In this case, the occurrence of the palpitation was evidently followed by increase in the size of both thyroid and eyes. Pulse equal and regular—116.



Bloodletting was adopted three times in the treatment of this case, and after a little time a leech was applied every fourth day to the enlarged thyroid; as in the former case, a temporary benefit resulted. The girl appears to have ultimately fallen a victim to tubercular disease in the chest, surviving the time of her first illness for nearly three years.

Taking the cases which have occurred in their own experience, and those which have been elsewhere recorded, there are in all twenty-seven which form the subject of the following interesting remarks by Drs. Romberg and Henoch. They acknowledge twenty-seven to be too small a number of observations to warrant any very decided opinions being drawn from them, but, at the same time, believe it sufficiently large to afford the groundwork for much useful study. By far the larger number of the patients were females, only four of the twenty-seven were males. All with one exception were young, the most common age being between twenty and thirty. In the larger number of the cases there existed the combination of the three symptoms of palpitation of the heart—enlargement of the thyroid and prominence of the eye; while in six of the twenty-seven cases one or other of the three was absent. In the two cases first treated by Dr. Romberg in the Clinic, the prominence of the eyes was not observable. Undoubtedly of these phenomena the palpitation of the heart is the one best understood, and corresponds most readily with the view taken of the whole disease. Almost always the cardiac symptoms are those first discovered and first complained of; then, after a longer or shorter period, the swelling in the neck commences, and the prominence of the eyes follows. Only a few of the recorded cases lead to the supposition that the three diseased appearances arose at about the same time, certainly in the fifth case treated in the Clinic the enlarged thyroid was the first symptom noticed. It is well to inquire wherein the original affection of the heart consists. That in some instances there exists organic disease is proved as well by examination during life as by post-mortem examination (Basedow and Marsh); but, again, in others it is equally certain that the cardiac symptoms depend merely on an increased irritability of the organ (Cooper, Begbie, Lubarsch). Cases explicable on both these grounds, and on these only, have been treated by the Clinic. In regard to the thyroïdal swelling, it is interesting to note its increase and subsidence after the violence of the cardiac palpitation—this fact is expressly stated by Sir Henry Marsh, and mentioned by Begbie as existing in his third case, and also noticeable at times in the fourth; this, of course, points to an intimate relation between the two symptoms. Marsh and Heusinger describe the condition of the thyroid as a true hypertrophy, and it has been noticed by Graves that, after the lapse of years, the consistence of the gland has been much increased.

As regards the remarkable prominence of the eyes, this symptom comes on gradually, and so far as vision is concerned it is not of much importance, seeing that only in one case (Lubarsch) was it at all seriously impaired; but, though sight is not much affected, the prominence of the eyes produces a singular disfigurement, causing the sufferer to be not unfrequently avoided in company. Acknowledging the great difficulty of determining upon what peculiar condition the prominence

of the eyes depends, our authors set aside the view of the increase of the aqueous humour causing a true enlargement of the eye (Begbie—the theory also adopted by Dr. Stokes), also that which attempts a solution by reference to an hypertrophy of the post-ocular cellular tissue (Basedow). They look upon the idea of the prominence being due to a want of tone in the ocular muscles, and an accompanying congestion in the posterior parts of the eye (Cooper, Dalrymple) as more likely; but they appear to think still more favorably of the view of Heusinger, who found in two cases an extraordinary accumulation of fat in the cellular tissue behind the eyes, and regards it as the probable cause of the exophthalmos.

Again, regarding the disease as a whole, our authors proceed to remark that certainly the larger number of the individuals so affected exhibited evident symptoms of anæmia, such as a remarkable paleness of the skin, the peculiar sound audible in the blood-vessels of the neck; headaches, often very violent; giddiness, especially when in the upright posture; humming sound in the ears; attacks of fainting; small, frequent pulse, &c. Irregularity of the catamenia also is commonly present, while fluor albus, and sometimes complete amenorrhœa, have been found. Symptoms of an hysterical nature further distinguished not a few of the cases, the globus hystericus, neuralgic pains in different parts, coldness of the extremities, and strange wanderings of the mind. Basedow describes a remarkable calmness and a great desire for pleasure as characteristic features of the mental condition. In some of the cases it is clear that if the disease was not originated, at all events it was furthered by the occurrence of a severe hemorrhage or flux, which reduced the system (Begbie); also a depressed state of both body and mind seemed connected with its first occurrence (Graves). But though anæmia was present in a large number of the cases, there are others whose commencement could not be traced to it, and anæmia cannot therefore be regarded as an essential requisite towards the explanation of the complex phenomena. The irregularity in the uterine system, too, cannot be regarded as altogether explanatory of the disease; for, independently of males being subject to it, these uterine derangements, though marked, were of very varying nature. Heusinger directs attention to the condition of the spleen, which he found after death much increased in volume and manifestly diseased.

(2) The case by Dr. Banks is valuable, in having the appearances after death as well as the symptoms during life.

CASE.—The subject of this case, a woman, æt. 30, was admitted into Whitworth Hospital, January 25th, 1855. Her health had been good up to the age of fifteen, and no hereditary taint was discoverable. About the age of puberty she suffered much mental disquietude, from which she has never since been altogether exempt. She does not remember the exact period when the catamenial function was established; it had always been irregular, and for the last year and half had ceased altogether, having been suddenly arrested in the midst of a period. She has always been nervous and subject to palpitations of the heart. Every winter for the last few years has had bronchitis; some time since she suffered from a fit of violent vomiting and straining, and after this she perceived that her neck was swelled, and she felt a sensation of



throbbing in it. Of late she has been much distressed by palpitations and pulsation in the neck, and a feeling as if she were being choked by something drawn tightly round her throat; for the last ten nights she has been almost sleepless, and utterly unable to lie down.

On admission she presented the following appearance:—Wild, agitated expression of countenance; dusky hue of skin; eyes unnaturally prominent, staring, and brilliant; evident enlargement of the thyroid gland, more particularly of the right lobe; violent throbbing of the vessels of the neck, which were considerably augmented in size; one large superficial vein crossed the trachea.

The thyroid, permanently enlarged, becomes much more turgid on the occurrence of palpitation of the heart, or paroxysms of coughing; a purring thrill, a loud continuous venous murmur, and an interrupted arterial sound are present.

The area of precordial dulness is increased; the heart's action is tremulous and irregular in the extreme; a few unequal beats of extraordinary rapidity, and then a brief pause. No murmur was distinctly audible.

The pulse is small, feeble, and unequal, and so rapid as to render it almost impossible to calculate its frequency; the countenance indicates great suffering. She says her chief distress arises from inability to lie down, or to sleep quietly, from a "feel as if her heart was in her throat." She also complains of headache and frequent cough; during the fits of coughing, her urine passes away involuntarily. With the exception of bronchitic râles, nothing abnormal was found by auscultation of the lungs. The heart's sounds were more extensively audible than is usual in health.

From the date of admission, January 25th to February 6th, no marked change in condition of patient; at the latter date, œdema of the lower extremities, and a slight puffiness above the eyes, were observed. Greater respiratory distress; restlessness and mental disquietude. The size of the thyroid is rather greater than when first seen; position has a remarkable effect on the pulsation; on assuming the recumbent posture, which always causes dyspnoea, the throbbing of the vessels visibly diminishes, and the murmurs become almost inaudible. The urine is of a dark smoky colour, albuminous, and depositing a sediment which, on examination, was found to consist of broken down blood-globules, sp. gr. 1·017. The heart's sounds more regular.

From this time the patient rapidly declined in strength; the œdema extended almost over the whole body; the cellular tissue of the back was the seat of extensive effusion; inability to sit up from weakness, and she says from the weight of her head, and giddiness. Still the pulse remains more regular (96). The character of the urine unchanged.

For the last week of life intense bronchitic râles were heard, and there was an abundant expectoration of a bloody fluid, not viscid.

It should have been remarked that the vision was perfect up to the close of life. There never was the slightest inflammatory affection of the eyes. Death, which occurred on the 7th March, was rather sudden. She had been speaking a few minutes before, and so free from struggle were her last moments, that those near her did not know exactly the moment of her death.

The morbid appearances presented on examination were in many respects interesting.

The thyroid gland, enlarged to four or five times its natural size, was found to cover, to a considerable extent, the front of the trachea. The right lobe was larger than the left, the thyroid veins were remarkably dilated.



The gland was dense, very solid to the feel, and lobulated. A section of different parts disclosed the existence of numerous cysts, containing a yellow fluid like honey. The contents of some of the cysts were dark coloured, and resembled coagulated blood. The microscopic appearances were similar to those observed and figured by Rokitansky in ordinary enlargement of the thyroid. The jugular veins were enlarged; the bronchial glands were found of an unusually large size; the heart was enlarged generally and the cavities dilated, but not to any considerable extent.

The heart was as large as that of an ordinary man, the subject being a woman under the middle size; the valves were free from disease, with the exception of slight thickening of the anterior edge of the mitral valve.

The lungs were highly congested, and the bronchial membrane bore the marks of intense inflammation.

The liver appeared as if in the earliest stage of cirrhosis; the spleen large and congested.

The brain softer than natural; the lining membrane of the ventricles much thicker than usual.

The kidneys had undergone the changes usually observed in the early stage of Bright's disease.

#### ART. 52.—*Remarks on Cardiac Disease.* By Dr. SEMPLE.

(*Medical Times and Gazette*, July 7, 1855.)

The following practical remarks upon the moral management of cases of cardiac disease, especially as affecting the valves, occur in a paper relating the case of a medical man who had long had valvular disease of the heart, with fatty degeneration, and who died from other causes at an advanced age:

“With regard to the moral management of such cases, I am strongly of opinion that too much importance ought not to be attached to the presence of cardiac murmurs in forming a prognosis. In a paper which I communicated to the Medical Society of London in the year 1850, I ventured to broach the somewhat heretical doctrine that disease of the heart, even affecting the valves, was not so certainly or prematurely fatal as was generally supposed, and among other cases I then adduced was the instance of an old woman who died of mere old age at the age of eighty-three, and in whom I was fully aware of the existence of extensive valvular disease for ten years before death. If, therefore, we should detect a valvular murmur in one of our patients, I do not see the necessity of at once proclaiming the fact, inasmuch as the fatal news communicated to the sufferer might tend to hasten the very event which it should be our duty to avert; and inasmuch also, as valvular disease may proceed to a very great extent without materially shortening the period of human life, or causing much suffering. I cannot forget the instance of an estimable member of our own profession, who, having been informed of the existence of a slight valvular murmur in his heart, poisoned himself immediately after receiving the melancholy intelligence. In the case which is the subject of the present paper, the patient had in some manner become acquainted with the fact of the existence of a slight valvular murmur; and as he was much given to hypochondriasis, and would lie awake

for nights together brooding over his own symptoms, I am aware that the knowledge of this murmur gave him very great and perhaps unnecessary uneasiness. I frequently endeavoured to comfort him by assuring him that the murmur was due to arterial degeneration, from which few persons of his years were exempt, and that the disease was not incompatible with the continuation of life or the enjoyment of moderate health. In effect, although labouring under fatty degeneration and valvular disease of the heart, which had probably existed for many years, he never suffered from any of the rational symptoms which are usually said to indicate those affections, and he died at a good old age, from an attack of bronchitis following a cold."

ART. 53.—*On protracted Valvular Disease.* By DR. CORSON.

(*New York Journal of Medicine*, May, 1855.)

Dr. Corson arrives at the following conclusions, after a very careful analysis of forty-one cases of protracted valvular disease which have fallen under his care:

1. That in protracted valvular diseases, aortic lesions are twice as numerous as any other lesions; aortic obstruction alone and aortic obstruction with regurgitation, nearly equally divided, forming about one half; mitral regurgitation, nearly one fourth; aortic and mitral combined, one sixth; and simple or complicated affections of the right valves, about one eighth.

2. That owing to the excess of three males to one female in the large aortic class, two thirds of the whole were males; and that the average age of the whole, rejecting fractions, was thirty-nine years; of the aortic alone, forty years, and of the mitral alone, thirty-two years.

3. That in both ability for exertion and *duration*, the aortic on the whole excelled; each being greatest with either slight aortic obstruction or limited stationary mitral regurgitation, with the *least hypertrophy*; enlargement being best endured in the aortic class; and that the average duration of the forty-one cases reached the extraordinary term of nine years; nine with aortic obstruction averaging ten and a half years; eight with aortic obstruction and regurgitation, eight and a quarter years; seven of aortic and mitral combined being ten years; and three of right and left valves simultaneously, being four years.

4. That of twenty-nine cases in which the causes were mentioned, twenty, or about two thirds, were from rheumatism, four from pneumonia, or pleurisy, and none from Bright's disease.

5. That the *aortic* differed from the *mitral* in having, more often, florid face, bright protuberant eye, jerking, thrilling, or firm pulse, precordial bulging and pain, strong impulse, harsh grating or sawing murmurs *always sternal*, more frequent and large hypertrophy, and more cerebral complications; while the *mitral*, on the contrary, were more liable to a livid or puffy face, softer, smaller and more intermittent pulse, feebler impulse, more liquid murmurs *always apical*;

less hypertrophy, and more frequent pulmonary complications and dropsy.

6. That in *prognosis*, from their presence or absence in these cases, were variously rated, with some exceptions, the following characteristics: *most favorable*, slight *aortic obstruction* or limited stationary mitral regurgitation with little or *no hypertrophy*, a natural or florid face, and a healthy easy avocation; *less favorable*, aortic obstruction and regurgitation, or *free* mitral regurgitation with large hypertrophy, excessive pulmonary congestion or dropsy, and an unhealthy straining or anxious avocation; *unfavorable*, mitral obstruction, tricuspid with left valvular lesions, or disease of any two sets of valves sufficient to produce murmurs from both during life; Bright's kidney, a livid or cachectic face, or enlarged liver.

7. That eleven cases at the time reported were still living; and that of the thirty fatal cases only seven—all purely or mainly aortic—were mentioned as dying “suddenly;” while sixteen, principally mitral, “sank gradually;” and that in the thirty post-mortem examinations, beginning with the most frequent, there were found the following cardiac pathological changes: hypertrophy with dilatation, ossific deposits or calcification, adherent pericardium, valvular thickening, cartilaginous induration, and vegetations.

8. That in the treatment of *protracted* valvular disease, we must avoid, as far as possible, thwarting the restorative operations of nature, by depressing, with bleeding or digitalis; these, when required by dangerous complications or secondary inflammations, being used cautiously, as in phthisis, and on account of less liability to fatal syncope, being best borne in mitral disease with sufficient impulse; that many cases only require careful regimen for years; that allowing a certain excess above the natural standard to carry on the embarrassed circulation, the *impulse is the true pulse of the heart*, indicating, especially when feeble, the need of support; that when any interference is necessary, tonic and soothing remedies are far the most frequently required; and that the most valuable tonics to sustain a failing heart are, first, *strychnia* or *nux-vomica*, in from one fourth to one third the usual minute doses long-continued, and next, preparations of iron, with mild sedatives and bitters.

ART. 54.—*Curious case of Vascular Disease.*  
By Dr. GULL, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports*, 1855.)

It seems scarcely credible that a person should live in the enjoyment of her faculties, and in comparative health, with all the main arteries of the head and neck, except the left subclavian, closed at their origin from the arch of the aorta. Yet such was the condition in the following case. It illustrates the perfection of the arterial anastomoses of these parts, and may be adduced as a striking instance of that law of conservatism and compensation which prevails throughout the whole organism, but is rarely exemplified so plainly as in the conditions of the vascular system.



CASE.—Mrs. P—, æt. 41, a stout woman of the middle stature, always had good health until the autumn of 1852. She was at that time living near the Bankside, Southwark, and in consequence of a high tide overflowing the lower part of the house, made great exertions to save her furniture, and was exposed to wet and cold for some hours. This was followed by an affection of the chest, and with difficulty of breathing, which never entirely left her. Her general health also became perceptibly impaired, and she was troubled with flatulence, tinnitus aurium, and a sense of pulsation at the back of the head, at times very violent. For these symptoms she took various medicines without relief. On the 2d of August, 1853, Mr. Stedman, of Union Street, Borough, who has kindly furnished me with the particulars of the case, was called to see her for the pain in the head, which she said was such as almost to drive her mad.

On examining her he found the pulse at the wrist on the right side, and in the carotids, indistinct. A soft systolic murmur was audible over the ascending portion of the arch. Bowels torpid. Urine normal.

On the following day she suddenly lost the use of the left side. At the time of the seizure there was no loss of consciousness, and the speech was but slightly affected. After a few days the power began to return in the paralysed parts, and the face recovered its natural expression. I was requested by Mr. Stedman to see her on the 26th of September, on account of the severity of the pain in the head. On examining the heart I could not at first hear the systolic murmur in the ascending part of the aorta, to which my attention was directed; but on changing the position of the patient over to the right side, it at once became audible. There was no pulse at the right wrist, none in the right carotid, and but a feeble one in the left. The head-symptoms were considered to be due to softening, from defective nutrition, and quinine was accordingly prescribed, in the dose of six grains, in a pill, every morning. The immediate effect was a marked diminution of the pain; but on the 2d of October it again became aggravated.

By time, and by the use of various gentle remedies and sedatives, regard being had to the nature of the lesion in the brain, she slowly recovered, and, except some degree of weakness in the left arm and leg, was in her usual health. She was unable to take much exercise, and often complained of drowsiness. On the 11th of April, 1854, she had a second apoplectic seizure, and so suddenly that she fell into the fire and was rather severely burnt. The insensibility lasted but a few minutes. A week after she had a third seizure, and remained comatose until she died at the end of four days. There were premonitory symptoms of vertigo, &c., for half an hour preceding this fatal attack.

*Post-mortem examination.*—Integuments loaded with fat, on the abdomen more than an inch in thickness. The left arm slightly less in circumference than the right. Heart normal in structure, but rather small. Arch of aorta dilated, and to some extent succulated; internal surface irregular from atheromatous deposit and puckering; middle coat irregularly thickened. The openings of the innominate and left carotid totally obstructed, their position indicated only by indented cicatrices. This obliteration of the vessels at their origin was caused by a fibrous structure, incorporated with the lining membrane and inner layers of the middle coat, and extending upwards but a short distance, beyond which the vessels had their normal size and structure. The opening of the left subclavian (the only pervious trunk from the arch) was rather dilated; the vessel in other respects healthy. The right vertebral was small; the other vessels of the head and neck normal, as far as could be ascertained; but a complete dissection was not possible under the circumstances in which the examination was made.

The pericranium was loaded with fat. The calvaria was remarkably thin, and the bones without diploë. Arachnoid transparent. No sub-arachnoid effusion. On opening the lateral ventricles, the right corpus striatum was found to be much wasted and of an ochrey tint. On section, a large irregular cyst was seen in the outer nucleus of grey matter, and a smaller one in the inner, the intervening line of white substance being intact. The optic thalamus on this side was healthy. On the left side the anterior cerebral lobe and the anterior half of the middle lobe were in a state of recent softening, extending inwards so as to implicate the greater part of the corpus striatum, and the outer two thirds of the optic thalamus on this side. The softened brain had lost its transparency, and in it were found a few "granule cells," and some irregular aggregations of oil globules, either scattered amongst the nervous substance or coating the smaller arteries, and producing an appearance apt to be mistaken for fatty degeneration of the vessels themselves. There were no traces of effused blood. The crura cerebri, pons varolii, and medulla oblongata, were small, but healthy. The cerebellum free from disease. The carotid trunks, and the other vessels composing the circle of Willis, were remarkably thin, and everywhere free from atheromatous deposit. The right vertebral was very small, the basilar being formed principally by the left.

The descending portion of the thoracic aorta was healthy. The abdominal viscera were not examined.

#### ART. 55.—*Reduplication of both sounds of the Heart.*

By Dr. AUSTIN FLINT.

(*Western Jour. of Med. and Surgery*, April, 1855; and *Medico-Chir. Rev.*, Oct., 1855.)

"The occurrence of a reduplication of one of the heart's sounds, especially of the first, though not very frequent, is one that most clinical observers have met with. A reduplication of both sounds is so rare, that the following case, which may be regarded as almost unique in the regularity of the phenomena and the completeness of the observation, deserves especial attention."

CASE.—George Nash, æt. 27, boatman, admitted into Louisville Hospital, December 16th, 1853. Excepting an attack of cholera eighteen months previously, he had enjoyed good health till seized with cold and cough six weeks before admission. Cough chiefly at night; soreness under sternum; no hæmoptysis, chills, or loss of appetite; bowels regular; never had rheumatism or acute thoracic affection. On admission: aspect not morbid, pulse normal, respiration slightly increased in frequency, skin and tongue normal, appetite good, bowels regular, no thirst, percussion of thorax clear, breathing movements equal on both sides, sibilant inspiration on both sides. Some dyspnoea for a few days, with palpitation from commencement of attack, requiring him to lie with his head raised. The treatment consisted at first in the administration of syrups of ipecacuanha, squills, and tolu, with a little sulphate of morphia.

On December 23d, seven days after admission, the pulse at the wrist was found too quick to be counted, "but the carotids can be felt and, enumerating in this situation, counting at the same time the heart-sounds, the number of ventricular contractions per minute is one hundred and sixty." No œdema or ascites. Chest well developed. No apex impulse of heart; a feeble diffused impulse felt just below the nipple, and both seen and felt at

the epigastrium; no heaving of the chest; the heart-sounds succeed each other so rapidly that it is difficult to state them, but they appear pure, and the first sound is shorter than natural.

On the 24th, the absence of pulsation in the jugular vein is noted.

On the 28th, Dr. Flint says: "On examination of this patient yesterday, I found a feeble vibratory pulse at the wrist, numbering eighty per minute. I counted it repeatedly, with similar results; on counting the heart-sounds, I found them apparently one hundred and sixty-six beats—i. e., tic-tacs, per minute. I repeated the comparison of the heart-sounds several times, with the same results. To-day I find the same contrast—viz., pulse eighty, and two heart-sounds repeated one hundred and sixty times per minute. Dr. Dickinson counted, without knowing the results of my enumerations, with similar results." The same ratio was observed by Dr. Hardin, on the 31st; by Dr. John Clark, on January 1st, and also on the 3d and 13th.

On the 3d, a faint, short, sharp bruit was noted below the pectoral muscle, with the systole at the right and left apex, but not at the base.

On the 13th, a bruit, supposed to be endocardial, accompanied the systole, uniformly higher in pitch than the whispered word *who*, and nearly as high as *R*.

On the 25th, œdema supervened, and the dyspnœa became more urgent. The bitartrate of potash, followed by the exhibition of Epsom salts, relieved the dropsy; and

On February 14th, the patient was discharged, feeling quite well; able to sleep in the recumbent posture, without dyspnœa on taking exercise; no cough, pain, or palpitation. "*The pulse and the two sounds of the heart are eighty-four per minute.*" Marked dulness on percussion was observed to the left of the nipple. No point of apex impulse was seen or felt, but a very feeble impulse is appreciable over an area about two inches in diameter. No bruit heard." He left the hospital, and undertook severe work, without experiencing any inconvenience; and when Dr. Flint saw him again, on April 12th, the pulse at the wrist, and the ventricular contractions, were eighty-four each per minute; the sounds of the heart were pure, no apex impulse was appreciable, the dullness on percussion extended an inch to the left of the nipple, and the general health was good. He was again seen by Dr. Flint, in February, 1855, when he continued well, and no physical evidences of heart disease were found, save that the area of dulness was somewhat increased.

"In considering the causes of the phenomena in this case, Dr. Flint suggests two theories—either that the heart beat in the usual manner, but that, from the weakness of every alternate beat, that was not represented by a corresponding dilatation of the radial artery; or that the reduplication was owing to a want of synchronism between the contraction of each ventricle: each ventricle, as it were, asserting its independence by separate action. Want of synchronism between the ventricles is the cause to which Dr. Williams and Skoda attribute the reduplication of the first sound alone, and it is difficult to assume any other view in reference to Dr. Flint's case: he himself does not express himself decidedly in favour of one or the other, although he inclines to the theory of synchronism. He remarks, that a fact which would be incompatible with it was, that the beats in the carotid artery were equal to that of the heart. 'It is indeed wonderful,' he says, 'that of the halved portions of the systolic contraction of the left



ventricle, only one should regularly be accompanied by a radial pulse, but it is, perhaps, quite as difficult to conceive that, in view of the arrangement of the muscular fibres of the heart, the ventricles should contract separately.' The only point that suggests itself is as to the possibility of the peculiarity of the heart's action, whether explicable on the one view or the other, having been determined in the first instance by the remedies employed, and especially by the sulphate of morphia."

ART. 56.—*On perforation of the Septum Cordis.* By M. HAUSKA.

(*Wien Wochenschr.*, No. 9, 1855; and *Medical Times and Gazette*, April 28, 1855.)

Professor Hauska, having had a heart sent him, as furnishing an example of the aorta arising in both ventricles, found, on examination, that the appearance of this being the case arose from the septum of the ventricles having become perforated. He takes the opportunity to draw attention to an anatomical fact, allusion to which he can nowhere find, viz.: that there is in the normal state a spot in the septum cordis, varying in size from a bean to an almond, entirely destitute of muscular substance; the two chambers being there separated only by the layers of endocardium that line them. Examining the septum from the left, after slitting up the aorta, we may remark a thin diaphanous spot, close under the angle formed by the convex borders of the right and posterior semilunar valves of the aorta, being closed above by a thin muscular bundle, coursing along the contour of the ostium arteriosum sinistrum. In the right ventricle the deprivation of muscular substance is covered by the end of the tricuspid valve; and so thin is the duplicature of the endocardium, that the lines and markings of the finger held under it can be seen through. M. Hauska observed the appearance himself only a year since; but since then he has found it in every heart (about 300) he has examined, of whatever age or sex.

This appearance is of great interest in a pathological point of view. In endocarditis, the endocardium becomes loosened and friable, and it is not seldom actually torn, as the rupture of the valves and the rapid formation of aneurism of the heart show. If such inflammation happened to attack this spot, a communication between the ventricles, by rupture of the endocardium, might easily result. On examining the heart sent him as an example of anomalous origin of the aorta, the ostium arteriosum sinistrum was found directed towards the right, as well as the left ventricle, while the swollen edge of the septum ventriculorum, covered with opaque and thickened endocardium, sloping from before backwards, was carried up to the middle of the orifice of the aorta. This condition necessarily arises as soon as the endocardium closing the aperture is torn. The blood of the right ventricle passes, in gradually increasing quantity, through the new opening, which becomes proportionally enlarged, and, owing to the simultaneous contraction of the two ventricles, the blood does not pass into the left ventricle, but immediately into the aorta, where it becomes mingled with the blood of the left side. This newly esta-

blished stream of blood from the right ventricle to the aorta, gradually forces the commencing portion of the aorta towards the right, so that at last the ostium arteriosum sinistrum is placed obliquely over the perforated septum, and with its orifice turned towards both ventricles.

ART. 57.—*On the Tic-tac felt by the hand as a means of diagnosis in Disease of the Heart.* By Dr. MELVILLE.

(*American Medical Monthly*, Jan., 1855.)

The various cardiac sounds, natural and morbid, may be easily distinguished by the sense of touch after a little practice, if this sense be of ordinary delicacy; and M. Bouillaud has been in the habit of employing this mode of diagnosis in his practice lately. Dr. Melville's object is to direct attention to this fact, and in doing this he goes into considerable detail. Speaking of the advantages of this mode of diagnosis, he says "as a corroborative method of diagnosis it possesses great advantages and opens up a large and interesting field of investigation to the pathologist. Nor is it without peculiar claims to our consideration, in those cases in which the formality of a stethoscopic examination becomes irksome or alarming to a patient labouring under heart disease; at all times they are anxious, nervous, and easily excited, sometimes prejudicially so, particularly when suspicions may have been awakened that the special form of disease with which they are afflicted is incurable.

"In such cases a preparatory examination by touch may pave the way to the more formidable or repugnant auricular and stethoscopic exploration, or may, if we acquire by practice the dexterity professed by M. Bouillaud, altogether supersede the necessity of the latter."

#### (D) CONCERNING THE ALIMENTARY CANAL.

ART. 58.—*On tuberculous Peritonitis in Adults.*

By Dr. KYBURZ, of Zürich.

(*Schmidt's Jahrb.*, No. 3, 1855.)

In bestowing especial attention upon this disease, hitherto little regarded, the author thinks to fill up a void in special pathology. His deductions are based upon five cases observed by himself; a sixth, furnished by Prof. Lebert, is also a source of reference.

From the consideration of the diagnostic signs of these cases, we see how uncommonly difficult it is to recognize this form of disease at its commencement. The disease begins, according to the author's observations, generally with a chill, which is then sooner or later followed by the abdominal symptoms. When the disease is in its early stage, and the deposition of tubercle consists in scattered granulations upon different parts of the peritoneum, and latter occupies the superior regions of the abdomen, the belly is puffed up and of a roundish form. If the tuberculosis is propagated from one point (in

four cases it was the ileo-cæcal region), the abdomen becomes irregularly distended, and has a doughy and resistant feel. If particular parts are especially involved, various intumescences can be felt, which may readily be confounded with lesions of the spleen, liver, and uterus. In connection with this deposition and progressive development of the disease upon the peritoneum, the most various and opposite symptoms are called forth, to wit, tension of the abdominal walls, prominence of the liver and spleen, so also of the heart, compression of the lungs and dyspnœa; the organs also become pressed downward into the cavities in which they most readily subside; thus the author found in one case the vaginal portion of the uterus crowded down even to the labia. Through the pressure of tubercular degenerated lymphatic glands upon the excretory ducts of the liver, or through pressure of the peritoneal sheets one against the other, since they include the gall-ducts between them, arises the icteric appearance and the accompanying coloration of the stools. Disturbance of the urinary secretion from pressure, is not a rare phenomenon. In one case sciatica originated from pressure upon the nerves. Pressure upon the vessels naturally produces disturbance of the circulation, thence œdema of the feet. If the vena cava becomes compressed, the veins appear swollen upon the distended abdominal walls. In one case coagula had been formed in the veins. The phenomena of vomiting, diarrhœa, or constipation, and loss of appetite, are results of the disturbed action of the organs, partly from pressure, partly from morbid degeneration of the intestinal walls.

In reference to the general symptoms, we can, according to the author, determine the following rules. There is usually a slight febrile condition existing, the pulse even in the morning being over 90 per minute, very often accompanied with colliquative sweats. We observe in the patient a progressive emaciation and loss of strength, as well as an alteration of the colour of the face, but rather paleness with circumscribed redness of the cheeks than the straw-yellow colour peculiar to carcinomatous affections.

The shortest duration of the disease was four weeks, the longest six to seven months. The issue in five cases was fatal; only a single patient left the hospital in a satisfactory condition, and the author doubts whether he was really cured. Death followed from gradual wasting, or from the occurrence of various complications. Ulcerations of the intestine were observed only in one case, perforation in none.

The treatment was the same as in every other form of tuberculosis, the administration of cod-liver oil a long time continued.

**ART. 59.—*Curability of Intestinal Perforation in Typhoid Fever.***  
By Dr. WOOD.

(*Philadelphia Medical Examiner*, April, 1855.)

The case which is here related occurred in the Pennsylvania Hospital under Dr. Wood's care, and it was thought of sufficient importance to be brought before the College of Physicians in Philadelphia.



T. C., a young man, about twenty-three years old, was admitted into the hospital on the 21st of November. His friends stated that, three weeks before his admission, he had been attacked with a chill, and had been unwell ever since. At the time of his entrance, he had all the characteristic symptoms of typhoid fever in its advanced stage; among them, a pulse of 128 in the minute and very feeble, a perfectly dry tongue, muttering delirium, diarrhœa, and tympanitic abdomen, with the rose-coloured spots and tenderness on pressure. There was also cough, with embarrassed respiration and evidences of pulmonary congestion. He was put on the use of oil of turpentine, and small doses of the blue mass, opium, and ipecacuanha, with emollient poultices to his abdomen, dry cups to his chest, and wine-whey, soup, and milk, to support his strength. On the 24th, his tongue had become moist, his abdomen less tumid and tender, and his pulse reduced to 115. So far as the fever was concerned, he seemed on the way to convalescence; but the pectoral symptoms had increased, and pneumonia was decidedly developed. The remedies were continued, with the addition of a large blister to his chest. But the pulmonic affection rapidly gained ground; the respiration was very much oppressed; the skin became cold and clammy, and the pulse extremely feeble; and life was sustained only by the administration of the most active stimulants. He died on the 1st of December, obviously of the disease of his lungs, the abdominal symptoms having almost entirely disappeared.

On examination after death, the upper half of the right lung was found in a complete state of grey hepatization, being everywhere infiltrated with pus, and so soft that the handle of the knife could be passed through it in all directions with very little resistance. The remainder of the right lung, and most of the left, were more or less congested. There could be no doubt as to the cause of his death. I was not present at the examination of the body; and, though the lungs and intestines were kept for my inspection, I had no opportunity of seeing the bowels *in situ*. Dr. Forbes, however, one of the resident physicians of the hospital, who made the autopsy, was kind enough to present me with the following account, in substance, of what he discovered on opening the abdomen.

Evidences of previously existing peritonitis were exhibited in small portions of semi-organized coagulable lymph adhering to the bowel and abdominal parietes, chiefly in the right iliac region, where, at one spot, it served to agglutinate the small intestine to the anterior wall of the abdomen. There was little or no liquid in the cavity, and no fecal matter. The adhesion was carefully separated by the thumb-nail, when a portion of the contents of the bowel escaped into the abdomen at the point of separation. On further examination, a perforation of the intestine was found in the middle of an ulcer, which appeared to be healing at its edges, as were several other ulcerated surfaces.

When exhibited to myself, the intestines were, as already stated, separated from the body, and had been laid open. A considerable number of healing ulcerated surfaces were visible along the ileum, in the situation of the aggregated glands; and, in the middle of one of these ulcers, near the ileo-cœcal valve, was an oval opening, about half an inch in length, quite through the bowel, with a smooth, rounded edge, which had certainly not been produced either by tearing or by the knife. There could not be the least doubt that it was a perforating ulcer. Near its edge, on the peritoneal surface of the bowel, was an adhering layer of semi-organized coagulable lymph, about an inch and a half in diameter, and here and there smaller patches, with some shreds adhering somewhat loosely to the bowel, at a little distance from the

surface of adhesion, showing that the inflammation had extended some distance beyond the outer limits of that surface.

Dr. Wood adds :

“The College will, I think, agree with me in the conclusion that here had been a case of peritonitis from perforation, though of no great extent. Before any considerable portion of the intestinal fluid had escaped, adhesion had taken place around the opening by means of the exuded fibrin, and further mischief was thus prevented. The inflammation had probably begun to subside before the patient entered the house ; and the 24th may be considered as the commencement of convalescence, so far as the peritonitis was concerned. I presume that the patient would have recovered, but for the supervention of pneumonia, which, in consequence probably of the general debility and bad state of the blood, passed very rapidly into the third stage, or that of suppurative disorganisation.”

ART. 60.—*Case of Sarcina Ventriculi.* By Dr. SEATON.

(*Lancet*, May 5, 1855.)

Commenting upon this case, Dr. Seaton observes that it tends to confirm the view taken by Dr. Todd, that the occurrence of sarcinæ in large quantities in yeasty vomiting is indicative of dilatation of the stomach, generally the result of pyloric obstruction, but, after analysing the published cases of the disorder, he does not think that this symptom is pathognomic of this lesion. He also says that the case shows very clearly that the sarcinæ may be formed even in large numbers, independently of fermentation.

The patient was a respectable tradesman, aged forty-eight, of moderate height, evidently thinned by disease, though never stout ; always of a strong constitution, and never having suffered from any particular disease till the one with which he was now afflicted. He had been married twenty-one years. He was a house-decorator, and had always worked very hard, feeling the better for so doing. His habits were temperate, but he had been irregular with regard to his hours of meals, from devotion to his work ; bowels generally regular, or easily regulated. For the last seven or eight years he had felt at times peculiar languor, and this had increased so much during the last two or three, that he had been occasionally obliged to give way to it, and to quit his work and repose for a while. About eight or nine months ago he first experienced symptoms of derangement of the stomach, manifested by loss of appetite and flatulence, with increase of the general weakness and irregularity of the bowels. After several months there came on acid eructations, occurring almost directly after each meal ; and soon after this, vomiting of the food taken, along with a green acid stuff (the words of his description). This occurred at first generally four or five times a day, after each meal ; but was somewhat mitigated by medical treatment, and, at length, about a fortnight ago, sank to about once a day, mostly in the evening. On some days within the last fortnight he has vomited twice or thrice ; sometimes he has passed an entire day without vomiting at all. But, whatever the interval, it always seemed that, when he vomited, he brought up all that he had taken since the last time of vomiting, so that the longer the interval, the greater was the

quantity ejected. The ejecta consisted always of the food taken, whatever that might have been, excessively acid, and with a fetid sulphurous smell. This fetor has increased lately, and he has observed for some time that the matter vomited has fermented on standing. He has no pain, but for the last month has had a heavy dragging sensation at the stomach. During the whole nine months since the dyspeptic symptoms first set in he has been losing flesh. The abdomen was well examined: it was quite flat and soft; there was no tenderness; no tumour nor sense of resistance anywhere; there was extended stomach resonance. Some of the matter vomited was examined: it was a brownish liquid, intensely acid, in a state of fermentation. On standing, it divided into a thick scum of brownish yeasty substance at the surface; a quantity of semi-turbid fluid beneath, and at the bottom a deposit of half-digested food. A little of the scum was put under the microscope, and exhibited abundance of *sarcinæ*. Although the history of the case, and the patient's general aspect, left little doubt of there being organic disease of the stomach, the discomfort arising from the vomiting and the fermentation was so great, that it was thought desirable to try the effect of hypo-sulphite of soda. This was accordingly commenced on the 4th of July, 1855, in the dose of ten grains, three times a day, in an ounce of infusion of quassia. This produced no effect on the frequency of vomiting, or on the quantity ejected, but it gradually diminished the acidity, the fetor, and the fermentation, and within eight days entirely put an end to them. Some of the matter vomited on the 18th was submitted to careful examination, without detecting any *sarcinæ*. He continued the sulphite to the end of the month without any return of the fermentation, but without any effect on the frequency of vomiting. He left town, and went to the sea-side for the month of August, during which he took no sulphite; but the fermentation did not return, except on one or two occasions, and then he attributed it to his having taken improper food. He returned at the end of the month, evidently sinking fast. A remarkable lull, however, took place in his symptoms a few weeks previous to his dissolution. The vomiting for several days had been almost incessant, and by the 8th of September was quite so, not a teaspoonful being retained for a moment on the stomach, while his strength was apparently ebbing away. But on that night he did not vomit at all, and had tranquil sleep. The next day he got up, and remained up nearly the whole day without vomiting, and for ten days he only vomited on one occasion, though he took a great deal of food. On the 18th he ate too heartily of improper food, and on the morning of the 19th vomited a large quantity, which fermented briskly. This state of things continuing till the 22d, the sulphite was given again, and the fermentation was at once arrested. The sulphite was discontinued on the 24th; the fermentation did not return. On the evening of the 27th he expired. On the 23d, for the first time, the vomit had the appearance of coffee-grounds.

An examination of the body was made after death. There were some old adhesions of the left lung, otherwise the thoracic and abdominal viscera were healthy, except the stomach; this was enormously dilated, and occupied the greater part of the abdominal cavity; its parietes were very thin, but healthy in appearance, except towards the pylorus, where there was, on the anterior surface of the organ, an ulceration larger than a crown-piece. This ulceration did not extend to the pylorus, though it did to within two or three lines of it; but the mucous membrane between it and the pylorus, and the whole circumference of the pylorus itself, were in a state of colloid degeneration. Only a moderately-sized quill could be passed through the pylorus.



ART. 61.—*The prevalent Bowel Complaints in the Crimean Expedition.*  
By Dr. SMART, Surgeon of H.M.S. *Diamond* (Hospital-ship), at Balaklava.

(*Lancet*, June 2, 1855.)

The author describes two forms of bowel disease that have prevailed in the forces of the Crimean Expedition during the last winter, and he gives cases typical of each. The first affection described is that kind of dysentery terminating in ulceration of that variety termed "Pustular," by Blane, and other authorities in the medical histories of former campaigns. The other form of disease is classed "Diarrhœa," the name, as observed by the author, of a symptom only, and not even persistent in the course of the complaint, and which cannot convey any idea of morbid conditions that terminate in general softening of the villous layer of the mucous coat of the intestines—a lesion that causes death, by destroying the absorbing power by which the chylous products of digestion are introduced into the system.

Reviewing, says Dr. Smart, the term of six months now expired since leaving the shores of Bulgaria, there is seen to have existed throughout a winter campaign an almost exclusive disposition to diseases affecting the organs of digestion and assimilation. This of itself the author considers would point to malific causes of a nature wholly different from those which ordinarily determine the relative frequency of diseases; because, under common circumstances of winter season, in a climate rather more temperate than that of the British isles, the maladies that affect the glandular system, the respiratory organs, and those of locomotion, would be expected to prevail. In estimating the causes which have led to the production of other forms of disease than those just alluded to, the author observes, that it should be recollected that when the army left the plains of Bulgaria to combat on the heights of the Crimea, the cholera was not extinct from its ranks; that during the passage by sea it had given ample proofs of its not having left them, and that after they were landed it burst out with epidemic violence immediately after the battle of the Alma, when the allied troops encamped upon the ground upon which the enemy had been routed. Cholera followed the armies to the encampments on the heights of Sebastopol and Balaklava, but with the advance of winter it declined, and before Christmas its intensity had become so reduced that perfectly developed cases of it were of rare occurrence. The author continues, when cholera and diarrhœa were on the wane, diarrhœa and dysentery complicated with jaundice became very frequent complaints. These were at first manageable complaints, but the operation of new malific agencies, such as deficient vegetable supplies, improperly cooked and salted meat, the overcrowding of men in the tents, the continued position in the trenches, cold, defective clothing, and other causes which are inseparable from winter campaigns, induced a scorbutic cachexy effecting a complete change in the general expression of the bowel diseases, which then assumed the forms of "camp dysentery" and the "lienteric diarrhœa," which latter the author regards as a continuous link in the chain of the last cases of cholera.

ART. 62.—*The use of Riding in Hæmorrhoids.* By Mr. J. RANALD MARTIN, late Presidency Surgeon, and Surgeon to the Native Hospital, Calcutta.

(*Lancet*, Oct. 6, 1855.)

“The benefits derivable from horse exercise, in the prevention and cure of hæmorrhoidal affections, are not generally understood; yet they are very great. Riding on horseback acts beneficially by accelerating the course of circulation along the veins, while it brings the sphincter and adjoining muscles into alternate action, thus contributing, the one action with the other, to prevent venous engorgement, and to impart tone to the relaxed muscles.”

ART. 63.—*The use of Oil of Male Fern in Tænia.*  
By Dr. GULL, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports*, 1855.)

In this paper we find an abstract of fifty cases which have been treated by oil of male-fern since the year 1851, and reference is also made to 200 cases which have also been treated at Guy's Hospital in the same way, during the same time, and with such results that the oil “is now esteemed at the hospital as the most convenient and effectual remedy we have hitherto had.” In the fifty cases related in the paper, the result was uniformly successful. The oil was given to children of from two to three years old, and repeatedly, without the slightest ill effect. It generally causes some degree of nausea, and not unfrequently slight vomiting, and it acts as a moderately brisk purgative in a period varying from one to twelve hours, so that it was not necessary to associate any other purgative with it. The dose was, for the most part, from one and a-half to two drachms, taken in a mucilaginous draught, but Dr. Gull thinks that one drachm would have been sufficient in the majority of instances.

Contrary to the opinion of Bremser, the result of these experiments is to show that the oil is as effectual a remedy in the tape-worm of England (*tænia*) as in the tape-worm of Switzerland (*Bothriocephalus*).

The paper also contains some excellent remarks which, at one and the same time, help us to know whether a worm is present, and whether the medicine has acted effectually. It is well known that the segments are continually ripening and falling off from the free extremity of the worm, and if, therefore, there are no segments in the motion after a certain time, we may be sure that no worm is present. Now this certain time may be fixed roughly at three months; and if, therefore, this time passes without any evidence of the worm in a person who supposes himself to be suffering from worms, or after a vermifuge remedy has been administered, we may be sure that there is no worm in the bowel.

ART. 64.—*Case bearing upon the causes of Tape-worm.*  
By Dr. R. W. CRIGHTON.

(*Edinburgh Monthly Journal*, June, 1855.)

This case is very interesting as showing the importance of attending to diet as well as drugs in the treatment of *tænia*, and also from the bearing it has upon the causes of the disorder :

CASE.—In December 1853, I was consulted by J. B., æt. 40, a calico-printer, on account of tape-worm, with which he had been troubled for six or seven years. On inquiring as to what means he had employed for his relief, I was much struck at the long list of medical practitioners under whose care he had been, but without obtaining respite for any length of time from his distressing complaint.

He stated that the remedies employed had generally been successful in bringing away portions of the parasite, but that after a greater or less time, never exceeding a few weeks, his symptoms, itching, pain, &c., had invariably returned.

The observations of Dr. Nelson on the development of *Tænia Solium* immediately occurred to me, and led me to ask him whether he was in the habit of eating animal food uncooked. After some hesitation, he admitted that he was, that he had acquired the practice in his native county, Lancashire, and that since his removal from it to Derbyshire, his complaint had increased much, owing, he thought, to his not having fish so frequently as before.

He assured me that the practice of eating raw meat was quite common among the Lancashire operatives, and seemed quite incredulous when told that it would be the origin of his disease.

Although he used both beef and mutton, he preferred the latter, and used more of it in a raw state. When questioned as to the frequency of his taking it uncooked, he allowed that he did so at least once a-week.

He had beside him many different medicines, supplied or prescribed by various practitioners, which from time to time brought away joints of the worm. I therefore ordered him no more vermifuge remedies, but a brisk purgative, as his bowels were sluggish, and one of the preparations of iron, on account of his being weak, and anæmic. I enjoined him also, if he wished to get rid of his tedious ailment, to avoid raw flesh in future. I did not see him again for some time, but learned from one of his employers, that his health had improved much, and that he was able to attend to his duties from which he had been laid aside.

On inquiring after him during the summer of 1854, I was glad to find him nearly free of his complaint, and during the present month (May 1855), he states that he has been completely well since the end of last summer, not having seen any portions of the worm since the beginning of September, and that he has entirely abandoned the practice of eating uncooked animal food.

The case narrated is an illustration of the advantages that accrue to practical medicine from the cultivation of pure science, and it shows that the importance of inquiring into the hygienic influences to which a patient may be subjected, is sometimes of greater consequence than the administration of remedies.



ART. 65.—*The anthelmintic action of Quinine.* By Dr. DELVAUX.

(*Presse Méd. Belge*, April, 1855; *Phil. Med. Examiner*, Aug., 1855.)

So early as 1764, Professor Van Doeveren, of Groningen, reported two very interesting observations on the anthelmintic properties of Peruvian bark.

The first case was that of a child, æt. 12, affected with tænia. Purgatives, calomel, assafœtida, &c., had been tried, but in vain. At last, an ounce and a half of Peruvian bark was given in four days. After having taken the powder, the patient passed an entire tænia with the head.

In the second case, the patient was a young girl laboring under fever. She took an ounce of powdered Peruvian bark, made into an electuary with simple syrup. Three round worms were expelled.

In a great many cases, adds Van Doeveren, physicians have given this febrifuge with the single idea of subduing fever, and without having the least suspicion of worms, and nevertheless it has brought them away [this circumstance led to the employment of quinine as an anthelmintic by the author]. Heister combined bark with mercury in his anthelmintic electuary, probably because he suspected the vermifuge power of the former.

I have collected upwards of forty cases of children affected with lumbricoid ascarides who have been radically cured of this affection with sulphate of quinia. The salt usually produces, at the end of twenty-four or thirty-six hours, several liquid motions containing these entozoa.

The sulphate of quinia is also most effectual in removing the ordinary ascarides (*oxyures vermiculares*). As is well known, these parasites are lodged in the fecal matters in the rectum, sometimes in the colon. They appear to imbibe a remnant of the chyle which serves to nourish them. They are expelled in a ball (*peloton*) with the fæces, or escape by themselves, causing intolerable itching, tenesmus, and other annoyances.

Injections of sulphate of quinia, repeated every evening for a certain time, are capable of completely destroying these entozoa.

I had twice an opportunity of administering sulphate of quinia for tænia, and in both cases the worm was expelled and was not reproduced. The first case occurred in October, 1855, and was that of a widow, æt. 28, who had suffered for many years from a tænia of which she was constantly passing one or more segments. Every known anthelmintic had been administered without completely freeing her from her malady. After having taken about forty-six grains of sulphate of quinia, she voided several yards of a tænia, the characters of which corresponded with those of the *bothriocephalus latus*. The medicine was continued for some time, and she has ever since enjoyed good health, and has had no return of the worm attacks.

The subject of the second case, dating from the month of March, 1855, was a little boy, æt. 4, who, according to the report of the parents, had been from the time he was one year old in the constant

habit of passing entire ells of a large flat worm, which I recognized to be the *tænia lata*. Sulphate of quinia was exhibited, a worm twenty-nine and a half feet (nine metres) in length was expelled, since which there has been no return of the affection.

Sulphate of quinia is, therefore, truly an anthelmintic. The physician often meets debilitated sickly young children, whose constitution bears the stamp of the most profound asthenia. He generally shrinks, when these children are at the same time attacked with worm affections, from the long list of anthelmintics, which, most frequently, only act on the digestive tube by producing violent effects, which are often felt injuriously throughout the entire system.

It is in such cases especially that the sulphate of quinia is advantageous, and I have never seen its employment followed by unfavorable consequences. Sulphate of quinia produces its vermifuge effects in virtue of its bitter properties; for bitters, as is well known, act more energetically as poisons on animals, in proportion as the latter are lower in the scale of creation.

Is it not on account of their bitter properties that Celsus and Cælius Aurelianus extol wormwood and centaury as anthelmintics, and that Riviere (*Praxis Medica*, Book v, chap. 9), praises the same and other plants as vermifuges, and as especially efficacious in removing lumbricoid ascarides? Kluyskens, in his treatise on '*Materia Medica and Therapeutics*,' says that "bitters are very detrimental to worms." "It is a very curious fact," observes this writer, "that vegetable bitters should in general be so destructive to inferior animals; flies perish almost immediately on being wet with an infusion of quassia."

It is, therefore, not impossible that sulphate of quassia should be capable of killing intestinal worms. Moreover, by its tonic action, it restores the power of the digestive organs, debility of which strongly predisposes to the production of entozoa.

The dose of the sulphate must vary according to the age of the patient; from two to ten years it will range between three and six grains; in older persons, so much as nine grains may be given in the twenty-four hours.

#### (E) CONCERNING THE GENITO-URINARY SYSTEM.

ART. 66.—*The pathology and treatment of alkaline conditions of the Urine.* By Dr. G. OWEN REES, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports*, 1855,)

The opinion held by Dr. Rees, and held by him for more than ten years, is that the existence of the phosphatic diathesis, as described by Dr. Prout, is more than doubtful; and that phosphatic deposits in the urine are simply due to the urine being rendered alkaline by the alkaline secretion of the mucous membrane of the urinary passages and bladder after it has left the secreting surface of the kidney. He holds, indeed, that *healthy* urine is secreted in the so-called phosphatic diathesis, and that when not healthy it is frequently more acid than

it ought to be. He holds, moreover, that undue acidity of the urine, when secreted, is very frequently the cause of that irritation in the urinary passages, by which these passages are made to throw out an unusual quantity of alkaline mucus, and thus undue acidity of the urine, when secreted, is a cause of undue alkalinity of the urine when excreted.

Dr. Rees states distinctly that his own experience is quite opposed to the belief that the urine is ever *alkaline* when secreted by the kidneys, except in those cases where it is made so by medicines or by diet, and he thinks that alkalies are the proper remedies for alkaline urine. The relief obtained in gonorrhœa from remedies which render the urine alkaline is, he thinks, a sufficient proof that the natural acidity of the urine will irritate an inflamed mucous surface, and this relief points at the same time to the propriety of neutralizing this acidity in cases where there is undue irritation of the urinary passages from any cause. This idea, moreover, is quite borne out by Dr. Rees' experience, and he is fully satisfied that alkalies form the proper remedies in the so-called phosphatic diathesis, and that there is no fear of any deposits being caused by them.

Nor is it any objection, Dr. Rees argues, that *acids* are occasionally of service in these cases, for these agents may tend to restore a healthy condition of the urinary passages by improving the general tone of the system. Under all circumstances, however, it is more easy to bring about a cure by alkalies than by acids.

"The mineral acids may improve the health, and they will not render the urine very much more acid and irritating: and therefore the patient may in time do well under their use, but their administration must interfere, in any case, with the exhibition of such remedies as are best adapted to relieve the state of the patient; and if the case be more advanced, even any trifling excess of acidity may inflict mischief. I have often watched the attempt to acidify alkaline urine by the exhibition of mineral acids; and after complete failure, have as often succeeded in restoring the healthy acidity by the alkaline treatment. One of the most striking instances I have seen of late, showing the advantage arising from remedies which render the urine alkaline *as secreted*, occurred lately in the practice of my friend Mr. Cock, who requested me to visit a young man who had undergone the operation of lithotomy. His symptoms were most unfavorable, the urine strongly alkaline, and the wound in the perineum (through which the urine still flowed) was completely coated with phosphatic deposit. In this case it was determined to exhibit the ammonia-citrate of iron and citrate of potash, in doses such as would render the urine alkaline *as secreted*, the patient being meanwhile supported by stimulus as before. In a few days a great change was brought about. The urine having been rendered less irritating to the inflamed mucous surfaces, they had begun to recover themselves, the phosphatic deposit was by degrees washing off, and every bad symptom had abated.\* The earthy salts seemed in this case to be acted upon by the urine, thus medicinally rendered alkaline; and it would appear not impro-

\* This patient was eventually discharged cured.



bable that the excess of carbonic acid and alkaline bicarbonate produced in this urine, owing to the decomposition of the tartrate and citrate in the organism, had exercised a considerable solvent action on the earthy phosphates. This was quite a case for a crucial experiment, and the result was satisfactory in the extreme."

The object of the paper is to recapitulate what has been already stated, and to state the results of subsequent experience.

ART. 67.—*On the nature of Diabetes.* By Dr. PAVY.

(*Guy's Hospital Reports*, 1855.)

The following interesting passage occurs in a paper entitled 'Researches on the Nature of the normal destruction of Sugar in the Animal System.'

"If I dared to hazard an opinion on the nature of that obscure disease, diabetes mellitus—the few observations, that I have yet made on the blood of these patients would lead me to say that there is a modification of sugar produced by the liver which is not susceptible of undergoing the normal process of destruction or metamorphosis in the animal system, and which is eliminated on its arrival in the capillaries of the renal organs. The experiments of Bernard have shown that if vegetable glucose (grape sugar) be injected into the general venous circulation, it is not destroyed in the system but is eliminated by the kidneys; whilst if it be injected into one of the veins of the portal system, and thus made to transverse the capillaries of the liver (so as to be converted into animal glucose), before entering the general circulation, it subsequently undergoes destruction in the system, and does not appear in the urine. Diabetic sugar would, therefore, seem to bear a resemblance in its physiological relations to vegetable rather than to animal glucose. But I am now engaged in a series of experiments on this subject, which, with the information we possess at the present time, I would venture to hope may some day lead to some definite conclusions concerning this long vexed pathological question."

ART. 68.—*The condition of the Liver in Diabetes.* By M. ANDRAL.

(*Dublin Hospital Gazette*, Sept. 15, 1855.)

Since M. Bernard's views respecting the glucogenic function of the liver were made known to the profession ('Abstract,' xix, p. 317), M. Andral has seen an abnormal condition of the liver in five cases of diabetes. The changes were in all identical, and consisted in a dark red coloration so remarkable, that the liver, instead of presenting two colours, yellow and red, presented throughout a uniform red colour; in fact, intense hyperæmia. This, Andral says, is an important fact, and if the liver secretes sugar, it is logical to admit that hyperæmia of that organ in diabetic persons is the anatomical sign of that over-activity which occurs in the glucogenic function. Thus, physiology and pathology mutually illustrate each other. In conclusion, says M. Andral, all I pretend at present to advance is, that in diabetes the

liver does not present its normal anatomical condition, that the changes found are always the same, and that this fact, taken in connexion with the discovery of the glucogenic function of the liver, may be considered as one of the proofs of its performing this function.

ART. 69.—*On the curative treatment of chronic Morbus Brightii.*

By Dr. HANDFIELD JONES, F.R.S., Assistant-Physician to St. Mary's Hospital.

(*Medical Times and Gazette*, May 19 and May 26, 1855.)

“Two conclusions, of the utmost importance to us in practice seem to me fairly derivable from the knowledge we have gained respecting Bright's disease. The first is, that the morbid condition, whether attended with hypertrophy or atrophy, is of the nature essentially of depraved unhealthy nutrition, not in any wise the result of ordinary inflammation attacking a previously healthy structure. In the atrophied kidney, I see a change just such as befalls any part that, from defect of its own vital energy, gradually decays. I have found recently just the same occurring in the pancreas. The enlarged kidney is, I am sure, frequently associated with scrofulous disease in other parts. I have seen it co-existing with tubercles and vomicae in the lungs, and deposit of bacony matter in the spleen, of a patient who died with tertiary syphilis. The enlargement of the kidney seems to take place much in the same way as the enlargement of a gland (lymphatic), which becomes the seat of scrofulous deposit. In both cases, unhealthy plasma is organized into low celloid forms, and in much the same relation to adjacent structures. The conclusion above stated is strongly supported by the latent, insidious manner in which Bright's disease usually comes on, by the efficient causes, and by the juvantia. To the former, as essentially of debilitating character, I have only space just to allude. When hyperæmia or inflammation actually makes its appearance, other symptoms are observed (especially in the urine) than those which occur in the degenerative state alone.

“The other conclusion is that, in a great majority of cases, in which the symptoms announce degeneration of the kidney, it may reasonably be anticipated that a considerable part of the organ remains in a state which is capable of restoration more or less complete. As long as the tubes are undestroyed, we may have hopes of being able to reproduce a healthy condition of their epithelial lining; if they have perished, the attempt must be ineffectual, but at any rate can scarcely be in any case injurious. We shall, therefore, do wisely to act on the most favorable supposition, and employ all our efforts to prevent the degeneration advancing further, to repair as far as possible the damage that has been effected. The question is, how shall this be done? and to this, of course, experience alone can give a satisfactory answer. Strongly convinced as I am that Bright's disease is not inflammatory, in any correct use of the word, but is purely a disease of depraved nutrition, I can entertain no doubt that the right method of treating it is to endeavour to improve the general vigour and power of the system, and therewith its nutrition, in every possible way. We must

not be satisfied with the removal of the dropsy, and restoration to apparent safety; but we must go on in the task of corroborating the system, till the urine has recovered its healthy condition, and the blood again imparts a ruddy hue to the complexion, and the muscles are toned to strength and vigour. I do not say that we shall always, or often, be able to do all this completely, but this is what we should perseveringly aim at; and I think we have good ground for believing that such persevering effort may make all the difference to many of our patients, between an early death and many years of tolerable comfort and enjoyment. I have seen a patient this very day who has *Morbus Brightii* in a marked form, with its perilous complications of dropsy, bronchitis, and threatening cerebral symptoms. His history is that he had smallpox at an early age, and has never been well since. How different might his condition now have been, had the renal degeneration, which no doubt dated from the debility induced by the smallpox, been observed and combated many years ago."

Dr. Jones then relates five cases, three of which we give, in order to show the beneficial effects of treatment. We quite agree with him, and we will say with him, "We are watchful to detect the invasion of phthisis, we combat its progress vigorously, diligently, and often successfully; why should we not do the same with this not less formidable malady?"

CASE 3.—F. P., widow, æt. 57, laundress, admitted Aug. 10th. Ill fourteen days. Abdomen and lower limbs swollen. Breath short on exertion. Pulse regular and steady. Sounds of heart natural. Clear breathing in the back. Tongue furred, with long papillæ. Bowels regular. Urine of pretty healthy colour, deposits a not abundant sediment, consisting of epithelial scales (many of them fattily degenerating), mucous corpuscles, or stunted particles of renal epithelium casts either homogeneous and containing a few corpuscles, or orange-coloured and made up of coarsely-granular and oily matter. There was also much scattered granular matter. Much albumen was thrown down by heat and nitric acid. Reaction acid. Sp. gr. 1015.

R Tr. Ferri Muriat., ℥ x;  
Tr. Digitalis, ℥ v;  
Inf. Quassia, ʒj, ter die.  
Pil. Hydr. c. Coloc., gr. v, alt. noct.

28th.—Getting on very well, swelling all gone. Urine pale, deposits a dark reddish brown sediment consisting of casts, mostly containing much molecular oil, with some corpuscles and blood globules. There are also very numerous perfect nucleated cells scattered over the field, and much diffused granulous matter forming films. It is decidedly, but not highly, albuminous; nitric acid only forms a cloud—no flakes. Reaction highly acid.

Sept. 4th.—Pt. in mist. omiss. tr. digitalis.

11th.—Complains of being heart-sick. Bowels costive. Pt. in Mist. Mist. Rhei et Magnes., ʒj o. mane. Urine at this date was scarcely altered by nitric acid; it deposited on standing a notable sediment of reddish hue, consisting of diffused granular matter, nuclei, granular corpuscles, blood discs, and a few glomeruli. These elements were sometimes united into masses. sometimes they formed complete casts of the tubes of some length. Hydrochloric acid precipitated no uric acid.

25th.—Better a good deal, no swelling, pain in forehead, which is hot for the last three days. Pt. in Mist., Olei Morr., ʒj ter die.



Oct. 16th.—Feels quite well, not the least swelling. Urine is wheyish, pale, sp. gr. 1014, gives no precipitate, not even increased opacity with heat and acid, but after standing some hours a very notable reddish sediment subsides, which on reboiling in great part disappears. A whitish sediment (not copious) is deposited from the urine on standing, which consists of granulous matter, nuclear corpuscles, and fragments of casts, most of them containing corpuscles and more or less oily molecular matter.

Nov. 13th.—Has remained quite well since last report. Discharged. The urine now was clear, palish, acid, sp. gr. 1018; it contained no albumen, no precipitate taking place after testing with heat and acid, and allowing the tube to stand some hours. Nitric acid precipitated a tolerable quantity of lithic acid, and when added to the concentrated urine plenty of nitrate of urea was formed. A slight sediment deposited from the urine contained a very few fattily degenerating casts, and some free corpuscles possibly of renal origin.

There can be no question, I think, that chronic degenerative mischief was going on in this patient's kidneys, when she first applied to me; and there seems good ground for the hope that this was arrested, and the functional power of the kidney, as well as its nutrition, increased, while it is certain that the general health was greatly improved and invigorated. Would these results have been attained by the administration of evacuates in any form? I think not.

CASE 4.—Samuel C., æt. 48, tailor, admitted November 14th. Is tall, robust, ailing last fourteen days, suffers with pain and tightness at chest; legs swell a good deal last few days; throat has been rather sore. Has pains in loins sometimes. Pulse rather jerky, weak. Tongue a little white. Heart's sounds healthy. Never had rheumatic fever. Is not in habit of drinking, has always had good health, but has been much confined last six months; used previously to have much exercise. Urine turbid, of smoky aspect, loaded with albumen, sp. gr. 1017, after separation of albumen by infiltration, sp. gr. the same, reaction acid; on standing it deposits a rather copious whitish sediment, consisting of corpuscles generally small, of numerous casts mostly pale and homogeneous, containing often small corpuscles or granular matter, and blood-globules. Considering that I had to do in this instance with congestion of the kidney superadded to degeneration, I had him cupped on the loins to  $\bar{3}v$ , and gave him Pulv. Jalap. co.,  $\bar{3}ss$ , o. mane, as well as Tr. Ferri Muriat.,  $\mathfrak{M}$  x c. Acid. Muriat.,  $\mathfrak{M}$  ij in Inf. Quass.,  $\bar{3}j$ , ter die.

On the 24th he stated that his breathing was a great deal easier; the swelling of the legs was less, but was still considerable; the abdomen was less swollen; the urine was a great deal more copious. The pulse was now full and forcible. He complained of having much violent pain in the head at times. Not yet feeling satisfied that the tendency to congestion of the kidneys was overcome, I thought it advisable to give him Ant. Pot. Tart., gr. ss, Potass. Acet., gr. x, in Inf. Calumb.,  $\bar{3}j$ , ter die, instead of the steel, but continuing the powder.

He continued this plan till Dec. 1st, when he reported that the urine was copious; he had to rise three or four times in a night; the pulse was quick, weak; the skin cool. The effusion in the abdomen had diminished, and there was less anasarca, but he had much swelling of the legs at night and of the face in the morning. Trusting that now I might safely return to the tonics, I gave him Acidi Nitrici,  $\mathfrak{M}$  v ex Infus. Gent. co., ter die; and Tr. Ferri Mur.,  $\mathfrak{M}$  x, ter die, c. Cibis, continuing the Pulv. Jalap. co. He improved immediately, the dropsy diminished, so that the legs appeared free from swelling, except a little at night.

This treatment was continued till Jan. 16th, when some more dropsical

swelling of the legs appeared, and the urine was found dark coloured and highly albuminous; he complained of stiffness in the loins, and had ten days before experienced some symptoms of catarrh. The pulse was 130 in the sitting posture; tongue clean; bowels open. Not wishing, if possible, to abandon the tonics, I changed the acid and steel for Hydr. Bichloridi, gr. i, Tinct. Cinchon., ʒij; ʒj ter die ex aquâ, with Quin. Disulph., gr. ijss, in pil. ter die.

On February 6th he had less pain of head; a slight degree of swelling of the legs had occurred at night in the last two days, but there had been none previously for three weeks. The urine continued very albuminous, but less so than it had been. I continued the pill, but combined now tr. ferri mur. with the bichloride, omitting the bark.

On the 23d there was only some trifling swelling of the legs; his aspect was pallid; the urine deposited a distinct but not abundant precipitate of albumen after testing with acid; it contained scaly epithelium, and mucous corpuscular forms in plenty, a very few casts (homogeneous and corpusculated), and a few blood globules.

March 16th.—He was gaining strength; the urine was rather cloudy, deposited a slight whitish sediment, was acid, and slightly albuminous, and of light colour. After treatment with nitric acid, it deposited abundance of uric acid crystals, sp. gr. 1015. The deposit consisted of scaly epithelium, with some renal, some doubtful blood globules, and a very few casts.

The same plan was persevered in up to April 24th, when he had been gaining strength pretty steadily; had some slight swelling of the legs, and some pain in head. The urine was very pale, and sp. gr. 1014, very slightly albuminous, and contained no casts. I now changed his quinine pill for one of Quin. Disulph., gr. i, Ferri Sulph., gr. ijss, ter die, and continued his mixture.

June 12th.—He reported himself improved, stronger a great deal, and had more colour. The urine was slightly turbid, contained a deposit of scaly epithelium, uric acid, oxalates in small quantity, and a very few casts, some of which were homogeneous, and others imbedded granular corpuscles. Reaction was acid. It was just clouded by  $\text{NO}_5$ , sp. gr. 1022.

By July 20th, the same plan being continued, there was not the least dropsical swelling, and the urine was not albuminous, but contained a good many casts.

August 10th.—The mixture was discontinued; he took the iron and quinine pills alone. He continued improving, finding himself not so well if he omitted his pills.

On Sept. 7th, the urine was examined at the hospital and noted to be of good amber colour and not albuminous, and the same was the case on the 28th, though he was complaining of rheumatic pain in the back.

However, careful examination of the urine at home, on October 12th, showed that after treating it with heat and acid, and allowing the tube to stand quietly for several hours, there was deposited a small reddish sediment of albumen. The sp. gr. 1030; the urine was of good colour, slightly cloudy; it contained a very few pale, homogeneous casts, entangling oil-molecules or corpuscles.

On November 6th, he resumed the use of quinine and sulphate of iron, which had been intermitted since September 28th; they were now given in rather larger doses, two grains of the former to three of the latter, ter die.

He gained in strength and general health, and on January 4th I discharged him, very tolerably well, though certainly with less colour in his face than I could have wished. His urine, then, I noted as not albuminous.



On November 6th, the urine was clear, rather light coloured, of sp. gr. 1026 ; deposited a very few casts, homogeneous and corpuscular, and some renal corpuscles. It was very acid. It was not perceptibly clouded by heat and acid ; deposited much uric acid in crystals after treatment with  $\text{NO}_5$ . After standing some time, with addition of  $\text{NO}_5$ , a slight precipitate remained at the bottom.

On Dec. 13th, which was the last close examination I made before his discharge, the urine was clear, of good colour, of sp. gr. 1028 ; contained no casts or renal epithelium ; treated with nitric acid it did not appear altered. After standing a night thus tested, there were numerous crystals of uric acid deposited, and the fluid was very slightly clouded, but this was probably from lithates, as it disappeared with heat. When the urine was concentrated and treated with nitric acid, plenty of nitrate of urea was formed.

I should apologize for the length of this case, did it not appear to me of great importance as proving the efficacy of persevering therapeutical efforts. There can be no question that the kidneys were affected seriously by degenerative disease, which I am inclined to think was of the hypertrophic kind. The influence of remedies was decided, but very gradual, and it is of especial interest to remark that it was only the tonics that procured real, steady improvement. A critical period in the history of the case was about Jan. 16th, when it was a question of abandoning tonics, and recurring to mere evacuates. Fortunately, the bichloride of mercury came in at this time with marked good effect, and enabled us to proceed with attempts at radical cure. Three weeks later I combined the chlorides of iron and mercury, continuing the quinine, and from thenceforward recovery went on uninterruptedly. The gradual but complete change which took place in the urine was very interesting ; its sp. gr. increased, the albumen disappeared, the colour improved, the fibrinous casts ceased to be formed, and almost every proof was afforded that the function of the kidneys was restored. I confess that had the man been a private patient, I should have insisted strongly on his taking cod-liver oil and courses of steel for many months to come, till the improved colour of his face, and the absence of any return of renal symptoms had convinced me that the cure was permanent. A sea voyage would also, beyond doubt, have been productive of good, if undertaken during the summer months, and in a warm latitude. These are, however, luxuries of treatment which the hospital physician has rarely in his power.

CASE 5.—As I by no means wish to make out too favorable a case, I will now mention an instance in which the same kind of treatment, though employed pretty steadily, failed to do all that I had hoped for.

Ann U., æt. 30, married, a tall, handsome-looking person, was admitted under my care, as an out-patient, January 12th. She had an easy confinement five weeks before ; the legs had been swollen before the accouchement, but did not diminish in size, nor (as she stated) did the abdomen, or but little. There was distinct fluctuation to be felt in the peritoneum ; the legs were a good deal swollen. Pulse large and excited. Tongue denuded and fissured. Bowels much relaxed last fortnight, twelve motions a day. Urine was pretty clear, rather pale ; sp. gr. 1015 ; highly albuminous ; contained numerous casts, homogeneous and corpusculated, one containing a glomerulus, and some renal epithelium. The sight of the right eye was very weak, but there was no visible morbid appearance. She was suckling her infant.

I gave her at first *Ol. Morr.* with *Tr. Ferri Mur. ter die*, and a daily purge of *Pulv. Jalap. co.*, hoping that, as the quality of the blood improved, the dropsy would decrease ; but no such favorable change took place, not even after a small cupping on the loins, and with the aid of a saline diuretic con-



taining digitalis. Elaterium was tried without benefit, and matters remained *in statu quo* about a month. I then gave her that well-known excellent combination of blue pill, squill, and digitalis, which immediately caused diminution of the dropsy; and under this she went on improving for three weeks, the mouth being slightly affected. I then gave her Tr. Ferri Mur. along with the pill, and soon after omitted the blue pill altogether.

By April 6th, she had improved so far as to be able to walk round the Serpentine; the ascites had almost if not quite disappeared, but the legs were still rather swollen. I made frequent examinations of the urine.

On March 8th, I found that the sediment contained numerous uric acid crystals, a sign which I regarded as favorable from having often observed it in the urine of patients recovering from scarlatinal dropsy.

On April 29th, the urine was clear, after having let fall a slight pale precipitate consisting of numerous epithelial scales, some pus-like corpuscles, and epithelial flakes; there were scarcely any casts to be seen. Its reaction was highly acid; sp. gr. 1014; its colour a light amber; it contained a notable quantity of albumen; uric acid crystals were formed on the side of the glass in which the urine stood. Although the urine was so highly acid, it was remarkable that on each successive drop of nitric acid being added a white cloud was formed, which again quickly disappeared, and it was not until a good deal of acid had been added that the white cloud became permanent. I have often noticed this, and believe that the explanation is to be found in the circumstance that the albumen in the urine exists combined with an alkali, soda (as in the blood serum) which requires to be completely neutralized before the albumen can be precipitated. After this I gave gallic acid, gr. v, ter die, and Tr. Ferri Mur., but without any improvement in the state of the urine. The bichloride of mercury with Tr. Ferri Mur. was also tried for some time, but, though her health and strength increased, the urine remained as before. The Liquor Ferri Persesquinit., with nitric acid, was not more effectual.

By July 20th, the dropsy had almost entirely disappeared, the sight of the right eye was much improved; she could use it well, though it was not quite strong. She then went into the country, with directions to continue the use of the Tr. Ferri Mur., which she did for more than two months, and returned in the beginning of November last, much better and stronger, and with a colour. There was rather some thickening of the skin of the leg than any œdema. She is now able to do her household work, and looks very well. I have not been able to induce her to continue the steel, though I have strongly recommended her doing so.

On her return from the country, in November, the urine was pale, of sp. gr. 1015, highly albuminous, gave a pale deposit consisting of epithelial scales, numerous uric acid crystals, a few homogeneous casts, and some masses of nuclear corpuscles, together with free nuclei and granular matter.

The improvement in this case, which was effected by tonics, was limited to the general system; the urine was altered very little. I am much inclined to think that the cause of the persistent albuminuria was rather a permanent change in the capillaries of the Malpighian tufts than any considerable degeneration of the renal tubes. These capillaries, in the healthy state, have the extraordinary power of filtering off mere water and salts from the blood which traverses them; in Bright's disease this power is impaired to a greater or less degree, and liquor sanguinis, more or less altered, drains off. This impairment of the filtering power of the Malpighian capillaries is not solely and constantly associated with degeneration of the renal tubes; it is found, *per se*, to constitute the essence of the affection termed chylous urine, which

can be arrested by styptic and astringent remedies (such as oil of turpentine and gallic acid), and in which, after death, no degeneration of the kidney is found to exist. Now it is quite possible that though the proper renal tissue may have recovered its healthy state, and secretes a fair proportion of urea and uric acid, yet the capillary membrane of the Malpighian tufts may remain permanently damaged, and thus liquor sanguinis will be continually draining off and mingling itself with urine otherwise healthy. Somewhat of this kind I conceive the pathological condition to have been in the last-mentioned case, as I scarcely think, if there had been any considerable defect in the depuration of the blood by the kidneys, the general system would have regained so much of health and vigour. It is remarkable that although the renal disease must, beyond doubt, have been in progress during the pregnancy, yet the confinement took place without any convulsions.

I had written the above before I examined the urine on February 24th; it was pale, wheyish, deposited a good deal of scaly epithelium, but few if any casts; it contained a good deal of albumen. Its sp. gr. 1015. Its general appearance led me to fear that I had been wrong in the opinion above expressed; but when I found that HCL precipitated a good deal of uric acid, and that the concentrated urine yielded with  $\text{NO}_5$  a fine specimen of nitrate of urea, I was confirmed in my view.

## PART II.—SURGERY.

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### SECT. I.—GENERAL QUESTIONS IN SURGERY.

#### (A) CONCERNING TUMOURS.

ART. 70.—*On Dr. Landolfi's treatment of Cancer.*  
By M. LASEGUE.

(*Archiv. Générales de Méd.*, May, 1855.)

SOME sensation has lately been caused by Dr. Landolfi in various parts of the continent, by his promulgation of a new mode of treating cancer. This mode consists in the external application of chloride of bromine made into a paste with liquorice powder, either alone or in combination with other caustics. One of his formulæ is made of equal parts of four separate chlorides—bromine, zinc, antimony, and gold. The chloride of gold is supposed to be especially efficacious in cases of encephaloid cancer; the chloride of zinc where there is much hemorrhage. At the same time, a small quantity of chloride of bromine is given internally, but no great stress is laid upon this part of the treatment.

The caustic is applied to the part according to ordinary rules, care being taken to prevent injury to the sound parts. The pain which results, and which is often severe and protracted, must be combated by anodynes, if necessary; and when it ceases, the part is to be treated with poultices or ordinary dressings until the eschar separates, which is usually from the eighth to the fifteenth day. If any cancerous point remains after the separation of the eschar, a small portion of the caustic must be applied to it. In the end, we have to deal with an ordinary granulating wound.

M. Landolfi is at present conducting a series of experiments at the Salpêtrière under a commission of inquiry, but the result is not yet made known. M. Landolfi is surgeon-in-chief in the Sicilian army, and lecturer on cancerous diseases in the Trinity Hospital at Naples.

ART. 71.—*Case of Galactocèle in an old man.*  
By M. VELPEAU.

(*Gaz. des Hôp.*, No. 58; and *Medical Times and Gazette*, July, 14, 1855.)

M. Velpeau, in his work on diseases of the breast, describes, under the above title, a rare affection of the breast, all the examples of



which had occurred in women. Recently, however, he has met with a case, in La Charité, in the person of a robust old man, æt. 75, which is probably unique. Brought in for injury to the head, his left breast was observed to be as large as the fist, presenting all the appearances of the breast of a woman of the same size. To pressure, which caused no pain, it imparted the sensation of a bladder filled with semi-fluid matter, and the skin covering it was quite healthy. The patient stated that the swelling had appeared several years since, after a severe illness, and had never caused him any inconvenience.

After an incision, M. Velpeau forced out, by repeated pressure, a glass and a half of whitish, inodorous, ill-matured matter, resembling clotted cream, the clots, which constituted a considerable part of the product, being of a yellowish white, and easily crushed. Examining the fluid by the microscope, Robin found from one eighth to one tenth to consist of fatty globules, differing in nothing from those of milk. There were also numerous lamelliform crystals of cholesterine, and some pus-globules. In the solid part of the tumour the same elements were observed, together with many granular bodies, analogous to those of the colostrum, the elements being held together by a considerable amount of semi-solid amorphous matter. The man died of bronchitis. No trace of either glandular structure or of a membrane of the cyst could be found.

ART. 72.—*On the microscopical appearances in Elephantiasis.*  
By Dr. ALLAN WEBB, of the Bengal Medical Service.

(*Indian Annals of Medical Science*, April, 1855.)

We take the following particulars from an elaborate paper on this disease, illustrated with many carefully-told cases, and containing details as to the mode of performing those elephantine operations which are so often wanted in Bengal for this elephantoid disease. Dr. Webb is speaking of *E. scroti*. Of this there are two varieties, the simple and the venereal.

“ Its substance, in both varieties, consists of mixed fibro-cellular and elastic tissue, which is tough and most condensed in the oldest or circumferential part of the tumour, whilst the softest and latest additions appear to take place nearest to the trunk, unless one portion arising in cells, another in nuclei, account for difference in density. This cutaneous outgrowth begins from the exudation of a common albuminous product very like white of egg, which makes cellular tissue œdematous, and usually organizes itself into nuclei, which afterwards become fibres, at other times after fever the fibres originate in nucleated cells.

“ Dr. Skinner, Surgeon to the Governor-General’s Body Guard, has very kindly examined both varieties, which are essentially the same structure. The following is his description:

“ “ It most probably has its origin in an inflammation of the cellular tissue, into whose stretched and enlarged areolæ a fluid is poured out capable of speedy organization. The part never again seems to

return to its former dimensions, in consequence of no absorption taking place in this lowly organized new structure, hence that firm, tough, and brawny character which a section of the older portion of the disease exhibits.

“ ‘The older portion of the tumour manifests, under the microscope, nothing but yellow and white fibrous tissue; the latter being very abundant.

“ ‘The younger part shows the same thing. Here are also to be seen white fibrous tissue in many stages of development, from the simple cell to the formation of fibre-nuclei of this tissue in excessive abundance; of various shapes, round, oval, and lengthened out, containing numerous granules or nucleoli. Those cells which are very much extenuated and drawn out have lost their nucleoli, and show only a bright interior.

“ ‘The fluid, which is very abundant in the meshes of the young or softer part of the tumour, contains a few blood and colourless corpuscles, and is exceedingly albuminous.’

“ ‘My observation of some hundreds of these tumours agrees with Mr. Paget’s description of them.

“ ‘I examined with the microscope three of these tumours, which I removed on the same morning in the Mesmeric Hospital, in 1842, immediately after their amputation, and whilst they were yet living and rolling under the action of the muscular fibres. The great mass of the tumour is formed by a development of subdartoid tissue. Its cells containing sometimes crystals of margarine, and always abundance of albumen; its envelope, hypertrophied skin and dartos. I had never then seen what was pointed out to me by my friend and colleague Professor Macnamara, namely, that smooth unstriped muscular fibres are also found in these tumours; a fact unknown, as we have seen before.

“ ‘But on various occasions, as well as that I now allude to, I found the tumours contract at the point irritated, upon pricking the white substance. This is now fully explained, as muscular fibres are found in this substance.

“ ‘Professor Macnamara examined many specimens most minutely of both the venereal and simple variety. His description is very characteristic, and is here added :

“ ‘The outer portion of the tumour immediately under the skin was very dense and fibrous; of a glistening white colour, but became less and less compact internally, till in its central portion it presented a semi-fluid gelatinous mass. Examined with the microscope, the dense external tissue presented all the characters of the true cutis, but more closely woven, and containing a more than natural predominance of the white fibrous element. The interspaces between the bands of tissue became larger towards the centre of the tumour, and many newly formed fibres presented themselves. The cells from which they had been developed were still plainly discernible, as granular bulgings in the direction of the length of the fibre.

“ ‘The semi-fluid tissue clearly exemplified the mode of growth of these tumours. It consisted principally of a densely albuminous fluid, exactly resembling, in external appearance, the albumen of

an egg; and, like it, coagulable by heat to a compact white mass. In this were to be seen fibres in all stages of their growth,—cells still preserving their original shape, others becoming pointed at their extremities, and about to lengthen into fibres; and fibres from neighbouring cells connecting themselves together.

“ ‘Blood-vessels ramified through the mass; groups of fat-globules were here and there scattered through the tumour.’ ”

“In one case I removed the tumour six days after an attack of moon-fever. Here Dr. Macnamara observed exudation cells nucleated cells in great abundance, changing into fibres, immediately under the skin.

“I have never seen in these tumours any fibrinous deposits in the veins,—any indication of phlebitis or lymphitis. In the excellent paper of my friend Dr. Wise upon this disease, he seems to have regarded inflammation of the veins as the chief cause of the malady *generally*, as well as of the disease in the leg. He says ‘that elephantiasis is produced by an inflammation of veins.’ ”

ART. 73.—*Results of operations in cases of Elephantiasis.*

By Dr. ALLAN WEBB, of the Bengal Medical Service.

(*Indian Annals of Medical Science*, April, 1855.)

In the paper already referred to we find the following passage. The italics are ours.

“The disease” (Dr. Webb is speaking of *E. scroti*) “is most striking and wonderful—tumours upwards of 100lb in weight are safely removed by the knife in a few seconds by one operation, and in from two or three minutes, preserving intact all the organs of generation; and lastly, and most wonderful of all, *the very process which heals up this enormous wound, during two months or more of granulation, radically cures the disease. If elephantiasis have been in the extremities, as well as the scrotum, the amputation of the scrotum cures the disease.*”

(B) CONCERNING WOUNDS AND ULCERS.

ART. 74.—*Fluid India-rubber as an artificial cuticle.*

By Dr. STILLMAN.

(*New York Journal of Medicine*, Sept., 1855.)

We anticipate great advantages in the adoption of Dr. Stillman's suggestion.

“About a year since,” writes Dr. Stillman, “I was presented by Mr. Armstrong with a bottle of the milk of the *Hevea* or Caoutchouc, in the liquid form in which it exudes from the tree. It is preserved in that state by the addition of a small proportion of free ammonia, and is now introduced as an article of commerce for manufacturing purposes, and, from my experience with it, I am impelled to call the attention of the profession to it, as a most useful contribution to our



*materia medica.* It is of the colour and consistency of pure milk (if my recollections do not deceive me), but becomes transparent as soon as dry. Owing to its great elasticity, it does not contract so violently as the collodion, it adheres closely to the skin, and allows entire freedom of motion and application to any extent. In burns it has an advantage over anything I have ever used, as also in erysipelas. An acquaintance with it by surgeons will lead, I do not doubt, to many valuable improvements, in surgical appliances."

ART. 75.—*A new way of applying pressure in certain wounds of Arteries.* By Dr. W. H. B. JONES, R.N.

(*Cape of Good Hope Medical Gazette*, Oct. 1, 1847.)

A case somewhat similar to the following is related in the article on hemorrhage in Samuel Cooper's 'Dictionary,' only in this case pieces of copper were used instead of pieces of lead. The lead, however, is obviously preferable, as being more pliable.

CASE.—"A young plethoric midshipman required to be let blood, for an affection of the head (distressing Cephalalgia). From the accumulation of fat, no vein could be perceived at the bend of the arm, but a puncture was made in the situation of the median cephalic. A thrombus formed, very little blood flowed, but syncope took place. In a short time a tumour was perceptible, accompanied with shooting pain along the arm, extending to the shoulder, with numbness of the fingers, and a feeble pulse. There was no pulsation in the tumour, until the following day, when it had attained the size of a pigeon's egg, and pulsated strongly; there was now considerable heat, with increased pain of the arm, and some stiffness of the elbow joint. A firm compress and bandage was applied, and the parts kept constantly wet with the Sol. Acet. Plumbi dil. No change took place in the tumour for three or four days, when a more perfect plan of compression was considered necessary. Three circular pieces of lead, of gradually increasing size, from that of a shilling to a crown piece, were placed accurately over the wound, and a further compress of lead, of three inches in length, and half an inch broad, was laid over the main artery above the bend of the arm. A bandage, moderately tight, was applied over the whole fore and upper arm, and the cold lotion was continued. The tumour was examined every three or four days, and was found gradually diminishing in size. In a fortnight it had disappeared perfectly, when he was discharged cured; and, at the end of two years, he continued quite well. It is needless to say, that during the cure strict rest and antiphlogistic regimen were enjoined. The artery wounded in this case, was not in the course of either the brachial or radial arteries, and was, therefore, supposed to be one of those anomalous branches which sometimes arise from the brachial.

ART. 76.—*On Bullet-wounds.* By Mr. DUIGAN, R.N., attached to the Naval Brigade lying before Sebastopol.

The following particulars are from a paper giving an account of the wounded in the recent bombardment of Sebastopol:

"Our advanced trenches being, in many places, within forty yards

of the enemy's rifle-pits, wounds of great severity were inflicted on both sides, as the force of the bullets was undiminished by distance. The orifices of exit, caused by the conical balls, more resemble shell wounds, in some instances, than a bullet aperture. In wounds about the head, especially, I have seen nearly the whole of the parietal bone carried away.

"Notwithstanding those jagged wounds from Minié balls, I have seen a soldier of the 41st hit by one on the nose, which caused as clean a wound as if done by a sharp knife. The nose was divided at the junction of the cartilages with the bones. The lower portion dropping down, but adhering by a good pedicle. It was brought together, as in hare-lip.

"Most of the wounds caused by the new conical bullets are, however, remarkable for the manner in which they plough up the soft parts.

"A soldier of the 33d was struck by a ball which made six openings. It passed through the right thigh, through the scrotum, and through the left thigh, where it escaped.

"The Russians use several kinds of bullets—one a solid conical ball, which belongs to the Liège rifle; another of a larger size and conical form, hollow at the base, with a small pillar, or nipple, standing in the cavity. It is surrounded by three lines. At the base, to guide the ball in its flight, there are two other smaller ones, modifications of this principle. The old round ball is also still employed. In some cases two of those round bullets have been found connected by a transverse wire, like bar-shot."

ART. 77.—*A memoir on Indolent Ulcers, and their surgical treatment.*

By JOHN GAY, F.R.C.S., late surgeon to the Royal Free Hospital.

(12mo, London, Highley, 1855, pp. 108.)

This small work is an amplification of a paper which was noticed in a former volume (Abstract xviii, p. 205); but not merely this, for in reality it is a valuable treatise upon ulcers and their treatment. The principal object, however, is to inculcate a new principle of treatment in the case of those ulcers which appear to defy all other methods of treatment. This is, first, to relieve the tension of the adjoining skin and other tissues, where that is opposed to the healing of a sore, by incisions at right angles to the line of tension; and, secondly, to cover the ulcer with new skin by a kind of plastic operation, where the obstacle to cicatrization is in the fixed condition of the edges of the ulcer. In this latter case, the edge of the ulcer is pared, and the knife passed under the surrounding skin so as to detach it from the subjacent tissues; then the skin from each side is made to glide over the ulcer, and the opposed edges are fixed together in the median line by hair pins; and, last of all, collateral incisions are made to relieve the tension if there is any stress upon the pins.

We are sure that no surgeon will consider the time ill-spent which he devotes to reading this unpretending but really valuable work.

## (C) CONCERNING FRACTURES AND DISLOCATIONS.

ART. 78.—*On Mathysen's Gypsum bandage.*

By Drs. GRIMM and JUNGKEN.

*(Annalen des Charité-Krankenh., Bd. v; and Medico-Chir. Rev., July, 1855.)*

Dr. Mathysen's bandage was employed in thirty-six cases of fractures of various kinds early in 1854. The bandage is prepared by stretching it upon a table, and well rubbing powdered gypsum into it on each side. It is then rolled up or otherwise arranged, according to the use to be made of it. Immediately before applying it, it is dipped in water or other fluid, the limb being protected by a flannel or other bandage prior to its application. Any portion of the bandage that is found not to have become wetted is moistened by a wet sponge. Flannel will take up twice as much gypsum during the rubbing than linen; but it is more clumsy, and not so easily applied. If a very firm, immovable bandage is required, some of the gypsum, in the form of a thin paste, should be applied during the last turns of the bandage. Its appearance is much improved by passing a damp sponge several times along it while still wet, and at a later period it may be smoothly polished by means of glass. To remove the bandage, it only requires to be again well wetted.

The reporters pronounce this bandage as the best of all hitherto invented, including those that most resemble it, as the starch-bandage, upon the following grounds:—1. The rapidity with which it hardens. 2. Its simplicity and easy application. 3. Its small cost. 4. The ease with which it may be removed,—the linen composing it being available, after twenty-four hours' soaking, for new bandages. 5. Its firmness and immovability render it suitable for the most oblique and difficult fractures. 6. From its rapid hardening and its firmness, it is well adapted for those cases which require extension and counter-extension to produce coaptation of the fractured parts. The position obtained remaining unchanged, apparatus of extension, so uncertain in operation, and so annoying to the patient, is not required. 7. The ease with which it is borne. 8. Its porosity. Cutaneous transpiration is not quite suppressed, and if the fracture be complicated by wounds, ulcers, &c., these are indicated by the discharges making their way through the bandage. 9. The gypsum bandage is a good conductor of heat, and a bladder of ice placed over some oil-skin, around the fractured part, takes effect in five minutes. 10. When the bandage is properly applied, the form of the limb is so well displayed, that any irregularity of the fractured part may be judged of externally. 11. Its handsome appearance and regularity distinguish it from all analogous bandages. 12. Fractures seem to unite sooner under its employment.



## (D) CONCERNING DISEASES OF THE BONES AND JOINTS.

ART. 79.—*Case of Cancer in the Bones.* By MR. R. W. SMITH,  
Professor of Surgery in the University of Dublin, &c.

(*Dublin Hospital Gazette*, March 15, 1855.)

This case is, perhaps, the most remarkable instance on record, of cancer in the bones, and its value is greatly enhanced by the comments which accompany it. It occurred some time ago, but the precise time is not stated.

CASE.—The patient was a female, æt. 63, of sallow complexion and a very unhealthy appearance. Four years before her death, and eight after the catamenia had ceased, she began to suffer from lancinating pains in the left breast, and a small, hard tumour formed near the nipple, and very soon became adherent to the skin, was uneven upon its surface, and after some time the axillary glands became enlarged and indurated. The tumour underwent no further changes for two years and a half, when a scab formed upon its surface, upon the separation of which a very superficial ulcer, not as large as a shilling, remained; this sore never increased either in extent or depth, and yielded but little discharge. In fact, during the remainder of her life, the diseased breast attracted no share of her attention, and the lancinating pains which were at first experienced, latterly ceased altogether. Severely, however, and almost unceasingly, did she suffer from pains, which she conceived to be of a rheumatic character, in all her bones; she had cough, and pain in the right side; her appetite failed, her sleep deserted her, and her flesh wasted away: she was for a long time before her death completely bedridden; she could not endure that any one should touch her; and her efforts to move herself in the bed were, upon several occasions, followed by fracture. She died, exhausted by pain and suffering, and in a state of complete emaciation.

The post-mortem examination, which I conducted with the greatest care, proved most interesting, and revealed an immense extent of cancerous deposit, limited to a single system; for, with the exception of a tubercle, about the size of a small nut, in the liver, the organs, in all the cavities were perfectly healthy; while, upon the other hand, nearly the entire of the skeleton, from the head to the feet, was pervaded by cancer.

In the anterior part of the frontal bone, there existed a firm, white, elastic tubercle, of the size of a nut, in the diploë: it had absorbed both tables, and penetrated the frontal sinus, through an opening in its posterior wall. A second and much larger mass existed more posteriorly, which had separated the inner and outer tables widely from each other; reduced them to an extreme degree of tenuity, and, in some places, totally absorbed them, and in such situations, the pericranium and the dura mater formed the immediate coverings of the tubercle: both these membranes were healthy. In several situations, the frontal and parietal bones were discoloured, and presented a slightly corroded aspect: and through all parts, so altered, the scalpel could be passed without difficulty into the brain; and when the calvarium was removed, the corresponding parts upon the internal surface of the bones were found similarly discoloured, and raised slightly beyond the level of the surrounding bone. The cancerous deposit had, likewise, perforated the occipital bone, and absorbed a portion of the petrous process of the temporal bone.

The left clavicle was fractured external to its centre, and the interior of the bone was occupied by scirrhus matter from its sternal to its acromial extremity.

The left humerus was broken in its centre, and the cancerous deposit filled the medullary canal from the head of the bone down to its lower extremity.

The right femur was fractured at the junction of its upper with its middle third: a longitudinal section of the bone exhibited a fine example of carcinoma occupying the cancellated structure and medullary canal. A large, circular, scirrhus tubercle was seen in the very centre of the head of the bone; another existed in the cervix, while from this down to the lower third of the shaft, the cancerous matter was deposited in one continuous mass: the compact structure became gradually thinner as it approached the seat of fracture, and a circular perforation had been produced by the tubercle which occupied the head of the bone.

In the left femur, the ravages of the disease were even more extensive than in the right: the cervix was broken, a considerable portion of it absorbed, and the remainder occupied by a mass of scirrhus structure, which was also copiously deposited throughout the remainder of the bone which, near its centre, was perforated by one of the tubercles.

There were no tubercles in the bones of the leg, but the medulla presented a most unhealthy appearance and colour, and its consistence was totally different from that which it possesses in the normal state.

The ribs contained numerous deposits of scirrhus in their interior, and several of them were fractured.

The disease occupied the spinal column from the lower part of the cervical region to the sacrum; the cancellated tissue of the bones had disappeared, and its place was occupied by a firm, elastic, scirrhus structure of uniform consistence, and of a roseate hue; there were here no separate tubercles, one mass of the morbid deposit filling the interior of the body of each vertebra, and in many places the compact structure of the posterior surface of the bones had been destroyed. The laminæ of the vertebræ and the spinous processes had undergone the same carcinomatous degeneration, and the vertical extent of several of the dorsal vertebræ was much diminished, some of them not being more than half an inch in depth.

The bones of the pelvis were entirely converted into a scirrhus structure. The ilium was perforated by large circular tubercles, which were in contact with the periosteum of the iliac fossæ, and in some situations the carcinomatous matter was deposited in a continuous sheet throughout the diploe. The ischium and pubis, which had suffered an analogous degeneration, were both fractured, and the morbid growth had even made its way into the acetabulum.

The characters of this heterologous deposit were precisely the same in all the affected bones; it was white, firm, tough, and dense in its texture, highly elastic, and cut like cartilage; it in no respect differed from true scirrhus as it is seen in the female breast. The osseous tissue in contact with it had suffered no alteration but such has resulted from absorption.

“The preceding case,” says Mr. Smith, “furnishes the most remarkable instance with which I am acquainted, of cancer of the bones, a disease, many points in the history of which are surrounded by difficulty and involved in obscurity, notwithstanding the elaborate nature of the investigations to which it has of late years given origin.

“Using the term in its widest acceptance, we meet with cancer in

the osseous system under several forms, and a great variety of circumstances; it occurs either as a primary or secondary disease; we see it limited to one bone or invading many, either simultaneously or in succession; we find it either with or without profound modifications in the osseous structure in contact with it; it is seen with tumour external to the affected bone, or the latter may preserve to the end its normal form and outline; it may invade the bone from its periosteal surface or perforate it from within; it may present every variety of colour and consistence.

"It is by no means my purpose, at present, to consider the disease under all these different circumstances; on the contrary, I wish to limit my observations to the affection as it presents itself in the remarkable case which has been detailed.

"We have here an example of the deposition of scirrhus in the osseous tissue, an occurrence which has been denied by a recent and distinguished writer:—'*Il est à remarquer que le tissu squirrheux qui se trouve fréquemment dans les autres tissus ne se montre pas dans les os.*'\* It does not, it is true (although the contrary is stated by Cruveilhier), occur as frequently as some of the other form of malignant disease, but, nevertheless, I have seen several examples of it, and I may remark that in many striking features it differs from the encephaloid tumour of bone; for instance, it is very seldom, indeed, that the scirrhus tubercle passes the level of the surface of the bone in which it has been deposited; nor has my experience furnished me with any example in which it formed a tumour obvious to the eye, no matter how superficially placed the affected bone may have been; while, upon the other hand, the growth of the encephaloid tumour is almost unlimited.

"Again, the true scirrhus tubercle does not convert the bone into its own tissue; it is not found to contain osseous spiculæ; it produces no organic change in the bony structure by which it is surrounded, which is simply absorbed '*molécule par molécule,*' until what is termed spontaneous fracture takes place, and in some cases this is the first and only indication of the presence of so formidable a disease in the osseous system.

"Moreover, I have never seen the scirrhus tubercle confined to a single bone, on the contrary, it usually invades a large portion of the skeleton, nor have I observed it independent of cancer in some other part of the body; but with regard to the encephaloid disease, although frequently it attacks several bones, yet frequently also, it exists in one alone, and without any other organ or structure being affected with malignant disease.

"It will be understood that in the preceding remarks I make no allusion to what some have termed cancerous infiltration of bone, a form of disease marked by pathological phenomena totally different from those which characterise the scirrhus tubercles, and in which the osseous structure seems to suffer a genuine atrophy, both of its earthy and organic constituents.

"In the case which has been described, there is nothing more remark-

\* Nelaton, tome ii.



able than the universality of the deposit, the complete cancerous infection of the skeleton, and this associated with so trifling an amount of malignant disease in other parts. I can offer no explanation of this singular preference shown so frequently to the osseous system in cases of mammary cancer, which, indeed, appears to possess, in a higher degree than cancer of any other structure, the power of contaminating the whole economy.

“Lebert found the osseous system diseased in fourteen, out of twenty-three, cases of cancer of the breast. I have never found any of the bones affected with scirrhus in cases of uterine cancer, but Cruveilhier has recorded an example of this disease, in which the frontal bone was perforated by a scirrhus tubercle developed in the diploe; and also one of cancer of the stomach, in which tubercles were deposited in the humerus, leading to the fracture of the bone. The results, however, of my own experience, agree with those of Mr. Stanley, who states that he has never seen hard cancer in bone as a primary disease, or in any cases where the primary cancer was situated elsewhere than in the mammary gland. It is, of course, true, that malignant disease frequently affects the osseous system primarily and solely; but in such cases the morbid growth assumes the form of cephaloma, osteosarcoma, &c. &c.

“It has been in the very chronic forms of cancer of the breast that I have most frequently seen the osseous system generally infected by scirrhus; but I had lately under my care in the Richmond Hospital a case where the mammary cancer was of rapid growth, and in which severe pains in the long bones and in the vertebræ left little doubt on my mind of their being contaminated by the disease.

“When situated in the diploe of the bones of the head, the scirrhus tubercle generally destroys the internal to a much greater extent than the external table, but it seldom passes the level of the bone, nor have I ever seen it produce symptoms of pressure on the brain, or of irritation of the membranes.

“The class of diseases of the osseous system to which the case which I have detailed is to be referred, constitutes a subject surrounded by difficulty and much obscurity, and most important in its pathological and practical bearings, but one upon the consideration of which it would be impossible to enter at present; but I hope, upon some future occasion, to have an opportunity of discussing fully the entire subject of the malignant tumours of the bones.”

ART. 80.—*On Secondary Inflammation of the Joints.*—  
By Mr. COULSON, Senior Surgeon to St. Mary's Hospital.

(*Lancet*, June 9, 1855.)

These inflammations occur during the course of other disorders. They are not accidentally associated with them, but evidently connected by some peculiar link with the primary affections, as is shown not only by the peculiar characters of the secondary diseases, but by the frequency of their occurrence during the course of the primary affection. The term, “secondary inflammations,” is applied by the

author to these diseases of the joints, in order to leave open the question of their nature; but he is disposed to affirm that more accurate and extensive investigations will enable us to ascend one step higher, and trace them all to blood-poisoning. Having related the details of a case of gonorrhœal rheumatism, the author observed that the points of most interest in connexion with these secondary inflammations of the joints are:—

1st. What are the primary diseases with which they are allied?

2d. What is the nature of these secondary affections? are they of rheumatic origin, as the name given to them generally would lead us to suppose, or are they specific inflammations?

3d. If specific inflammations, does each group acquire its specific character from the primary disease on which it depends, or can we trace the whole class of secondary joint-affections to one general law, giving to all the same character, independently of the particular disease from which the group appears to originate?

According to the author, the chief primary diseases or conditions with which these secondary inflammations are connected may be divided into seven groups. They are—

1. The puerperal state, giving rise to puerperal rheumatism.
2. Exanthemata, especially smallpox and scarlatina, producing inflammations of the joints generally attributed to rheumatism.
3. Injuries to the genito-urinary apparatus of the male.
4. Gonorrhœa, followed by so-called gonorrhœal rheumatism.
5. Animal poisons, especially that of glanders.
6. The state of new-born children.
7. Injuries, amputations, &c., followed by purulent inflammation of the joints.

In speaking of puerperal rheumatism, the author pointedly alludes to the error committed by many writers, who join the name of rheumatism to the articular affections which occur in puerperal females. These differ from true rheumatism in every essential particular,—in the general and local symptoms, in the course, in the result, and in the effects of remedies. The general symptoms of acute rheumatism are inflammatory; those of puerperal arthritis are eminently atonic. Besides this, the general symptoms which accompany the articular affections of puerperal women do not belong to the joint-diseases; they do not correspond to any known class of fevers, but they depend on a peculiar state, which has been traced to purulent infection of the blood. The general signs of the rheumatic diathesis are absent in these and other cases of an analogous kind,—a circumstance which should be decisive of the question. The course of the two affections is different; although the local symptoms are much less violent in puerperal arthritis, it runs a much more rapid course than rheumatism, however acute. The local symptoms are altogether disproportionate to the effects produced on the joint, supposing the disease to be rheumatism. Acute rheumatism hardly ever ends in suppuration; whereas effusion of pus within the cavity of the joint is the main character of the puerperal disorder. The results are different; for rheumatism of the joints, *per se*, never proves fatal; whereas nearly all the cases in puerperal women terminate in death. Reme-

dies, therefore, produce no effect in the latter diseases; while rheumatism, in all its forms, is amenable to treatment. Puerperal arthritis may occur after parturition, or after abortion, during the early period of pregnancy. It may, or it may not, co-exist with puerperal fever; and hence the great diversity of general symptoms observed in different cases. Sometimes the articular disease is merely one of the effects of uterine phlebitis, the general symptoms being those of purulent infection of the blood. In other cases, we have uterine phlebitis followed by puerperal fever, and complicated with purulent absorption. Here the two orders of general symptoms—viz., those of puerperal fever, and those of purulent infection—co-exist. The secondary puerperal inflammations of joints have a tendency to run a very rapid course. The purulent effusion sometimes occurs within a few hours after the first symptoms of pyæmia. Several joints are attacked in succession. The cartilages are apt to suffer, being often softened, abraded, or absorbed. Effusions of pus often exist around the joint, and in the centres of the muscles; and the limb is frequently affected with an œdematous swelling, which much resembles phlegmasia dolens. In the great majority of cases, the inflammation is purulent; more or less pus is effused within the cavity of the joint, and the synovial membrane is injected, though it may happen that the injection is very slight. In other cases, the purulent deposits take place outside the joints. In a few cases, the articular inflammation is non-purulent, although deposits of pus are formed in the neighbouring muscles. Finally, in some cases, the inflammation of the joints is simple and slight, terminating of its own accord in a few days.

The next group noticed by the author, is that connected with injuries to the genito-urinary system of the male. The secondary affections of this group are usually purulent, though often simply inflammatory. The pus deposits exist very often exterior to the joint, as often, perhaps, as in the joint itself. The course of these inflammations is irregular; in some cases they are very acute; in others, the series of attacks, though sub-acute, is spread over a long period of time. Here the pyæmia appears to be of a chronic kind; and the secondary articular affections may terminate favorably. It is remarkable that many of these diseases appear to be produced by mere irritation of the urethral membrane; but Mr. Coulson thinks that, in such cases, ulceration or phlebitis has existed in some part of the genito-urinary system, the irritation merely acting as an exciting cause of absorption of the pus.

The articular disease, improperly called gonorrhœal rheumatism, is next considered; after which the author notices the well-known group connected with wounds and injuries.

Inflammation of the joints connected with blood-poisoning from the introduction of certain animal poisons is then described. Mr. Coulson connects this group with glanders, the only poison whose effects on the joints has yet been studied. The articular inflammation which appears during the course of glanders is generally purulent, though sometimes simple. Its principal character is chronicity; the blood seems to be affected by many successive poisonings, and hence,



perhaps, the reason why the articular inflammation is occasionally simple, although the primary disease is essentially purulent.

The secondary joint-diseases connected with smallpox might be placed in the preceding group; but, in compliance with received opinions, the author classes them under the exanthemata. These variolous inflammations are sometimes purulent; but they are commonly slight, and terminate spontaneously in a few days.

Scarlatina is also attended by a peculiar inflammation of the joints, which has latterly attracted much attention, although its history is still obscure. Many physicians persist in regarding it as rheumatic; but while Mr. Coulson admits that epidemic rheumatism may co-exist with epidemic scarlatina, he believes that most of the cases which have been described as rheumatism are really secondary inflammations of the synovial membrane, of the kind described in this paper.

From these considerations, the author is disposed to infer that the seven groups of secondary inflammations of the joints, which he has described, may be all referred to one specific cause,—viz., infection of the blood. Moreover, he attributes five out of the seven to purulent infection of the blood; while he attributes variolous and gonorrhœal inflammations to the same cause, though with a certain reserve, as not yet fully established. The circumstance of these inflammations being often simple, Mr. Coulson says, is not conclusive against their connexion with blood-poisoning, because the articular inflammation consequent on pyæmia is not invariably of a purulent nature.

#### (E) CONCERNING OPERATIONS.

##### ART. 81.—*On Synchronous double Amputations.*

By Dr. MARSDEN, Governor of the College of Physicians and Surgeons of Lower Canada.

(*Montreal Med. Chronicle*, June, 1855.)

Dr. Marsden writes upon this subject, partly to settle a question of priority, but principally to enforce what he considers to be a good rule in surgery,—a rule which all army surgeons especially should have their attention directed to at the present time. The rule is, for two surgeons to operate simultaneously in those unhappy cases in which it is necessary to remove two limbs. This rule is allowed in Lower Canada. It was first laid down and put in practice, many years before chloroform was introduced, by Dr. Morris, in the Hôtel-Dieu, at Quebec, his assistant being Mr. Hall; and again, in 1837, in the same institution, by Drs. Parrant and Sewell. Since this time, Drs. James Douglas and Sewell have operated at the Marine and Emigrant Hospital in the same place; and last of all, Dr. Wolfred Nelson, of Montreal, whose paper on the subject has given occasion to the article under consideration.

Dr. Marsden says:

“My own conviction is, that recovery is more rapid under the simultaneous double operation than under the double interrupted amputation. It is obvious that, by the removal of both limbs at once,

the nervous irritation that the unamputated limb occasions is done away with, and the mental disquietude that the patient always suffers in anticipation of the second operation is avoided, besides saving the time that is usually necessary for the patient to recover his strength and tone after the first nervous shock. But more than this, the saving of the vital fluid tends materially to the rapid recovery, as well as the abridgment of the duration of suffering. The quantity of blood lost in the double simultaneous operation is little, if any, greater than in each single amputation of the same member ; and, in this view, I am supported by the opinions of all the gentlemen who have operated, or assisted at these operations.”

ART. 82.—*The treatment of purulent Ophthalmia.*  
By Mr. FRANCE, Ophthalmic Surgeon to Guy’s Hospital.

(*Guy’s Hospital Reports*, 1855.)

In this paper, several cases are related, in order to show the benefit resulting from the treatment of which the synopsis is subjoined. A table of results is also added which speaks very pointedly to this effect. The treatment is now uniformly adopted in Guy’s Hospital.

*Table of Results of Cases of Purulent Ophthalmia.*

28	Persons were affected with the disease.
43	Eyes were affected.
7	Eyes were completely spoiled before admission.
36	Eyes were subjected to treatment in hope :
3	Of these were lost, the cornea having been hazed or ulcerated before admission ;
*1	Was lost, respecting which the state of cornea on admission is not recorded ;
1	Remains under treatment, with fair prospect of recovering useful vision ;
1	In the same subject as the last, has already regained excellent vision ;
30	Exclusive of that just particularised, were saved, retaining perfect, good or useful vision. Several of these had been contaminated by gonorrhœa, and in several ulceration had commenced before admission. The group comprises <i>all</i> in which the cornea was intact on admission, unless that marked * were an exception.

*Synopsis of Treatment.*

- a. During the acute stage :
  - 1st. Local depletion by leeching the lids and scarifying their inner surface, every twenty-four hours.

- 2dly. Scarifying the ocular conjunctiva, according to Tyrrell's mode, in radii, daily, so long as chemosis is high.
  - 3dly. The application between the lids every three, four, or six hours, of drops of a solution of nitrate of silver, containing from three to eight grains in the ounce of distilled water; the strength of the collyrium and frequency of use depending on the severity of the symptoms, and being consequently modified as these abate.
  - 4thly. Constant fomentation and ablution with decoction of poppies, having a drachm of alum dissolved in the pint.
  - 5thly. The brisk exhibition of calomel and antimony after proper relief of the bowels, until the chemosis is subdued, or the mouth gives the earliest signs of commencing mercurial action. This remedy must be guardedly used, when the stage of active interstitial deposit, marked by rising chemosis, has given way to that of ulceration manifested in the cornea. It should then be accompanied by a tonic regimen and by—
  - 6thly. The use of quinine, in cases where debility prevails at the outset, or is subsequently induced.
  - 7thly. Moderately nutritious diet, which seems generally most eligible from the commencement, and may be progressively improved as the disease recedes.
- β. In the convalescent or chronic stage:
- Tonics in diet and regimen, varied local astringents of mild character, and counter-irritants must be used to complete the cure.

ART. 83.—*On sympathetic inflammation of the Eyeball.*

By Mr. TAYLOR, Surgeon to the Central London Ophthalmic Hospital.

(*Medical Times and Gazette*, Oct. 28 and Nov. 4, 1855.)

This affection occurs under certain conditions and at intervals varying from a few weeks to many years after the destruction of the primarily affected eye by inflammation, generally traumatic, but occasionally idiopathic. It is generally held that it occurs only after traumatic inflammation, but this opinion is refuted by the first four of the following cases. As to the cause, Mr. Taylor observes, that "all we at present appear to be warranted in asserting is, that pathological changes do occasionally take place within eyeballs which have been destroyed by idiopathic or traumatic inflammation, whereby products are generated which appear to act as foreign bodies; or, possibly, in some instances, to have a poisonous effect analogous to that possessed by the contents of a fluid cataract." According to this pathology, then, it is quite intelligible that ordinary means of treatment should prove to be altogether ineffectual, as they are proved to be, in this affection; at the same time Mr. Taylor does not agree with Mr. Pritchard, of Bristol, or Mr. Critchett, in thinking that extirpation of the organ is necessary.



“When we consider that the alternative is total blindness, even this severe operation would be not only justifiable, but imperatively necessary, were it not possible to attain the object equally by milder measures. But as I believe that in such cases the source of irritation lies wholly *within* the disorganized eyeball, I am induced to hope that the comparatively trifling operation which proved so successful in the eight cases detailed below will be followed by the same fortunate results on more extensive trial. The object of the operation is not, as Dr. Jacob asserts, ‘to lay open an eye with the view of causing its entire destruction by suppuration.’ It is not called for except in cases where the eye has not only been already completely destroyed, but is, in the vast majority of cases, a source of positive disfigurement. So far as the experience in this hospital goes, the offending cause is readily and effectually dislodged on the removal of the cornea. In no one instance has suppuration followed; on the contrary, the wound has healed with great rapidity, and, in all, a stump has been left admirably fitted for the support of an artificial eye,—an object, it must be remembered, of as great, or even greater, importance to the skilled artisan or the domestic servant, as to those in independent circumstances. I would not be supposed to speak dogmatically on the subject, because much more extensive experience is necessary before any positive conclusions can be arrived at; but I think that a good case has been made out for the further trial of the mode of treatment, which, it may be observed, was suggested many years ago by Mr. Wardrop and Dr. Mackenzie, though it does not appear to have been carried into effect by either of these distinguished surgeons.”

We think that Mr. Taylor does very good service to ophthalmic surgery by this excellent paper, for the operation here recommended is very simple and easy of execution—no small consideration for those who are unaccustomed to operate on the eye—and it is incomparably less severe than extirpation.

The following cases were under the care of Mr. Walton :

CASE 1.—George Watson, æt. 14, lost the right eye in infancy from disease, the symptoms of which he is not able to describe. The eyeball is slightly shrunk, the cornea clear, the situation of the iris occupied by lymph of a mottled red and yellow colour. He has had pain in the eye from time to time, more severe during the last few months. During the last twelve months, the sight of the left eye has been failing; there is severe pain, intolerance of light, and lachrymation, and the vision is so imperfect that he cannot read the largest print. The pupil is irregular, and adherent in several places to the capsule of the lens. The iris is dull, and the eyeball inflamed.

The cornea, and the lymph which lined the anterior chamber, were removed, and a mass, consisting of the capsule and part of the lens, converted into a chalky material, was extracted. The wound healed rapidly, the pain and inflammation of the left eye immediately subsided, and at the end of five weeks, when he ceased his attendance, he could read “minion” type with ease, though the iris had not altogether recovered its brightness, and the pupil was permanently disfigured by adhesions.

CASE 2.—Rebecca Wilson, æt. 7, lost the right eye in infancy, probably from purulent ophthalmia. The cornea is replaced by a small conical staphyloma. The eyeball is inflamed, and occasionally painful. The sight of the

left eye is much impaired; objects can only be seen when looked at sideways, and "a mist frequently passes before the eye." Not a day is passed without paroxysms of pain. There are not any objective symptoms of disease in this eye.

The cornea was removed, and a cretaceous capsule, enclosing a partially cretaceous lens, was extracted. Two months afterwards, when she was seen for the last time, the sight of the left eye was completely restored.

CASE 3.—A. H., a female, *æt.* 32, has a small conical staphyloma of the right eye, of the cause and duration of which she can give no intelligible account, further than that the sight of the eye has been completely lost for many years. For two years she has suffered from frequent paroxysms of pain in this eye. During the last few months the sight of the left eye has been failing; now she cannot see small objects distinctly, and a mist at times passes before the field of vision, obscuring everything. She is quite incapacitated from work of any description.

The staphyloma was removed, and the capsule, enclosing part of the lens, both loaded with cretaceous deposit, was extracted. Two months afterwards, the date of the last notes of her case, the sight of the left eye was perfectly restored.

CASE 4.—The notes of this case have been unfortunately lost; but the following particulars, given from memory, may be relied on:

The patient was a woman, *æt.* about 35. One eye had been lost, from idiopathic disease, many years previously; it was partly shrunken, and the pupil was closed by lymph. The sight of the other eye had been failing gradually for two years. There were luminous spectra and the appearance of a gauze veil before the eye, besides a total loss of the power of adjustment, so that the eye was almost useless, and she was quite unfit for any employment.

The cornea of the primarily diseased eye was removed, and the lens, converted into cretaceous matter, and firmly adherent to the subjacent parts, was extracted. The wound healed in a few days, and the recovery of the other eye was complete in about two months.

The following cases occurred under Mr. Taylor's care:

CASE 5.—J. Laxton, *æt.* 35, lost the right eye fourteen years ago, from slow disorganization, consequent on a punctured wound of the sclerotica, within the orbit. He felt no further inconvenience from the accident till about six months ago, when the wounded eye again became troublesome. He has occasional pain in it, but never very severe; there is excessive intolerance of light, lachrymation, and spasm of the eyelids. The left eye soon began to suffer in the same way, and, with the exception of pain, to an equal extent. As he cannot fix the eye on any object, it is impossible to say with certainty whether the sight is actually impaired. He has been incapacitated from work for three months, and the eye is of little use except as a guide.

The right eye is soft and atrophied. The cornea is clear, but little more than half the diameter of the other. The iris is discoloured, and pressed against the cornea by a body of a dull yellow colour. There is considerable sclerotic and conjunctival injection. In the left eye there are not any objective symptoms.

I removed the cornea and extracted the lens, two thirds of which were converted into a solid cretaceous mass, and the remainder loaded with oil-globules, scales of cholesterine, and molecular cretaceous matter. The chalky mass adhered very firmly to the subjacent textures. The wound healed in a few

days, the sympathetic disease subsided rapidly, and, in a little more than a month, he was able to return to his work.

CASE 6.—E. Moss, æt. 13, wounded her right eye with a pair of scissors, the point of which penetrated the cornea at its edge, and ruptured the capsule of the lens. Three months afterwards the eye was soft and atrophic; the iris was discoloured and adherent to the cicatrix of the cornea; the pupil was filled up by a plug of lymph. She had scarcely any pain, but there was excessive intolerance of light, spasm of the eyelids, and lachrymation. The left eye was so weak that she could not use it except in the dusk; she could not see to read ordinary print; and a mist frequently passed before the field of vision. These symptoms were becoming rapidly more severe, in spite of treatment. There were not any objective symptoms in the left eye.

I removed the cornea, and extracted the capsule, thickened, loaded with cretaceous matter in masses and small granules, and enclosing the *debris* of the lens in a similar condition. The wound cicatrised in a few days; the sympathetic irritation had almost completely subsided the day after the operation; and in a fortnight the eye had completely recovered its strength, and vision was perfect.

Dr. Garrod kindly analysed the cretaceous deposit, and ascertained that it consisted of phosphate and carbonate of lime.

CASE 7.—Susan Emberson, æt. 31, received a penetrating wound on the sclerotic margin of the left cornea from the bursting of a lemonade bottle ten years ago. The sight of the eye failed slowly and almost painlessly, and was not wholly extinct until nearly two years after the accident. For the last four years she has had an uneasy sensation of weight and pressure in this eye, increased to sharp pain by looking suddenly upwards. During the last twelve months these sensations have recurred with perfect regularity on alternate days, the eye being quite free from uneasiness in the intervals. During the same period the right eye has suffered sympathetically; on the “bad days,” as she terms them, she is quite unfit for any occupation; on the intervening days she cannot work or read for more than a few minutes at a time without everything becoming dim and confused. Her health and spirits are much impaired by anxiety, as she fears that total blindness is impending.

The left eye is shrunken, and grooved by the action of the muscles. The cornea is much atrophied, but clear. The iris, dull and discoloured, bulges forwards apparently from the pressure of a yellowish substance which occupies the posterior chamber, and adheres to the margin of the pupil. There is slight injection of the sclerotic and conjunctival vessels. There are not any objective symptoms in the right eye.

I removed the cornea and extracted the yellow substance which was seen through the pupil. It was about the size of a split pea, of the consistence of soft cheese, and was composed of amorphous molecular matter, scales of cholesterine, oil-globules, and a few fragments of the lens-fibres, but did not contain any cretaceous deposit.

The symptoms were much alleviated, but not completely removed, by the operation, which I have now reason to think was imperfectly performed. The disease, however, was rendered much more amenable to treatment; and now, after an interval of five months, has almost completely disappeared, under the influence, apparently, of full doses of iodide of potassium.

CASE 8.—J. Holdsworth, æt. 42, an engineer, received a severe blow on the right eye seven years ago, from a piece of steel an inch and a half in length, which broke from a spring which he was fixing in a vice. The blow was on the closed eyelids, and the eyeball did not appear at the moment to have been injured; but the sight began to fail soon afterwards, and was



totally extinct in about twelve months. The eye remained rather irritable, and subject to slight attacks of inflammation when he was exposed to cold, but gave him no further annoyance until ten months ago. He was then seized suddenly with agonizing pain in the eye, radiating over the whole side of the head and face. This has continued without alleviation or intermission up to the present time; he is rarely able to get more than two hours' sleep at a time, and even then, is never unconscious of pain, so that he is quite worn out with suffering and want of rest. The left eye has suffered sympathetically for four months; the sight is so much impaired, that he cannot read the largest letters on the title-page of a book, and the power of adjustment has been almost completely lost; there is great intolerance of light, so that the eye cannot be fully opened except in the dusk, and in full day-light he can barely see his way in well-known neighbourhoods. He has undergone a great variety of treatment without the slightest relief, and is anxious to have the eye extirpated.

The right eye is somewhat atrophied, and of a conical form from the bulging of the sclerotica in front. The cornea is clear, but smaller than that of the other eye. What appears to be the remains of the iris is in contact with the cornea, retains no trace of its natural structure, and is of a muddy ochre tint. The sub-conjunctival vessels are large, tortuous, and distended with dark-coloured blood; there is also some superficial injection. In the left eye there are not any objective symptoms.

I removed the cornea, and with a pair of forceps lifted out a clot, composed of an aggregation of granules, somewhat resembling those which are seen in congealed honey, but of a deep ochre colour. This was followed by a quantity of the same substance in a fluid state, which flowed out when slight pressure was made, until about half of the contents of the eyeball had escaped, when the vitreous humour began to appear. Along with the cornea, transfixed by the needle which was used to steady the eye, there was removed a solid body about the size of the crystalline lens, of a greyish colour in front; posteriorly, of a deep red. Smart hemorrhage followed the escape of these morbid matters; this was readily checked by cold, but left a large clot so firmly entangled in the wound, that it could not have been removed without the risk of completely evacuating the contents of the eyeball. This delayed the healing of the wound for a few days; but, in less than a week, it came away of itself; cicatrization then took place rapidly, the pain ceased entirely and permanently, and he enjoyed the first sound sleep he had had for nearly twelve months. The left eye regained its strength gradually; in about three months it had completely recovered, and he was able to resume his work as usual.

Mr. Quekett kindly examined the morbid contents of the eye. The solid part consisted chiefly of amorphous molecular matter, with a faint appearance of fibres irregularly interwoven; the ochre-coloured granules of the clot, and of the fluid which subsequently escaped, presented no trace of structure; they were composed entirely of amorphous molecular matter, which dissolved completely on the addition of acetic acid, as did also portions of the solid body, which was examined. Mr. Quekett suggested that this deposit might be the remains of a clot of blood, undergoing a peculiar form of degeneration; but, not having previously met with anything exactly similar, he declined giving a positive opinion as to its nature.

ART. 84.—*On the non-existence of Aquo-Capsulitis.*  
By Mr. WALTON, Surgeon to the Central London Ophthalmic  
Hospital.

(*Medical Times and Gazette*, May 5, 1855.)

After citing passages from various authorities describing the supposed disease, Mr. Walton proceeds :

“ I shall not offer any remark on these passages, as I think that the best answer is to be found in reverting to the anatomy of the parietes of the aqueous chambers.

“ On the membrane of Descemet, that is behind it, is a single layer of tessellated epithelium, which is limited to the cornea, and is the only true epithelium found on these walls. The front of the iris has not any, nor has the front of the capsule of the lens. On the posterior part of the iris, the uvea, there are, it is true, pigment-cells of the nature of an epithelium, but this evidently does not concern the present question. What then, you will ask, is the disease to which authors refer when they speak of aquo-capsulitis? What is the subjective symptom concerning the morbid anatomy of which they have erred? It is the object of this lecture to prove that it is merely corneitis.

“ You will find that the symptoms said to be pathognomonic of ‘aquo-capsulitis,’ are a mottled appearance, or peculiar kind of opacity, on the back of the cornea (considered most characteristic), muddiness of the aqueous humour, more or less disease of the iris, with occasionally closure of the pupil, increased vascularity of the sclerotica and conjunctiva. Let me analyse these.

“ In inflammation of the eyeball we do meet with opacities at the posterior part of the cornea, arranged in a peculiar manner. We see milky spots, varying in size from that of a pin’s-head to a degree of minuteness that evades detection by the naked eye, and, please to notice particularly, of the nature and precise seat of which we have no certain knowledge; one theory ascribing them to punctiform depositions of lymph in the posterior elastic lamina, or between it and the true cornea, either of which is likely; another, that is truly very extravagant, to tuberculous deposit in the epithelium behind the elastic lamina. But spots or opacities invade also other parts of the cornea, and occur at all depths, and therefore in all its textures, although the more anterior of them are less definite in outline, and generally less white. Can there, however, be any difficulty in attributing this slight dissimilarity to the anatomical arrangement of the part? You know that the membrane of Descemet, and its epithelium, differ from the anterior elastic lamina and the epithelium over the cornea. Surely we do not require to admit the existence of a serous membrane to account for this difference.

“ I find that those who dwell most on ‘aquo-capsulitis’ lay great stress on the back of the cornea being often alone diseased. But this is only partly true, since the disease when so situated is generally only in its commencement, and a close examination will, in most instances,

detect more disease in and about the eye than was at first suspected. Again, we as frequently, or more so, find vascular opacities and ulcer on other parts of the cornea, remaining long isolated. Unquestionably, the most common state of things is for the greater part of the cornea to become more or less diseased after the posterior part has been thus invaded. Minute ulcerations on the surface, general opacity or vascularity, or all these, with different degrees of vascularity of the conjunctiva and sclerotica, supervene. It follows, I think, of necessity, that in all cases of corneitis implicating the entire thickness of the cornea, that there must be posteriorly the punctiform patches. The anterior opacity necessarily obscures them more or less. I have often, under such circumstances, looked in vain for them with the naked eye, and even with a condensed light, when with a lens of high power I have distinguished numbers of them from the more anterior spots. I must beg you, however, to observe the fact, that the front of the cornea may be diseased and ulcerated for months, without the back being affected.

“Let me pass next to the ‘muddiness of the aqueous humour, and the implication of the iris.’

“There is no more common error in the diagnosis of ophthalmic diseases than when there is inflammation of the cornea, for it is to be supposed that the aqueous humour is turbid, and the iris dull and diseased. This is in no slight degree due to the theory abroad, about the chambers of the eye being lined by a serous membrane, but chiefly to the fact, that all objects viewed through a cornea, more or less opaque must necessarily be more or less obscured. I assure you, after no inconsiderable acquaintance with diseases of the eye, I am not familiar with turbidness of the aqueous humour, coexistent with transparency of the smallest portion of the cornea, except where a degenerated lens that has become fluid has escaped, after an operation, from its capsule, and admixing with the humour, produced discoloration.

In slight primary attacks of corneitis of a subacute or chronic form, where it has been possible to ascertain the actual state of the iris, I have never known this structure to be diseased. Numerous are the instances in which I have pointed out the immunity to students, when at a first glance it seemed very dull, and the pupil adherent, a spot less opaque than the rest, or a clear spot near the margin of the cornea having enabled me to correct the deception. Many times when the cornea has been generally too dull to permit a clear view of its state, and to all appearance it has been diseased, the quick and complete dilatation of the pupil under the employment of atrophine has proved that the cornea alone was in an abnormal condition. I have even seen deposits of what I supposed to be lymph at the margin of the cornea, in tubercles at four points corresponding to the vertical and transverse axes, and the iris remain healthy. Bear in mind, however, that disease of any part of the eye may be consecutive to chronic corneitis; the unhealthy action may, so to speak, travel backwards; but I believe that this is rare.

“When, however, there is acute inflammation of the cornea, and this for the most part is due to traumatic causes, the iris is frequently



involved. There is then inflammation of the eyeball, certainly of its anterior part, and now any inflammatory product may be thrown out in the anterior chamber. Conversely, the cornea is frequently affected in iritis of constitutional origin, and in all degrees, from a few spots on its posterior part, to disorganizing inflammation; and consecutively also in other diseases of the eyeball. I suspect that very frequently, when, among the remains of inflammation of the eyeball, there exist punctiform opacities at the back of the cornea, it is inferred that the primary affection has been 'aquo-capsulitis.' I have very recently had sent to me for an opinion, by Mr. Shaul, of Docking, a young lady, whose eyes would likely enough be thought to exhibit an excellent example of this. In the right eye, which was affected nearly three years ago, all activity of disease has passed away, and there remain at the back of the cornea the well-marked, opaque, characteristic spots; adhesion of the entire pupil to the capsule of the lens; a lead-coloured iris, and a discoloured sclerotica. In the left eye, which was diseased at a later date, there is similar implication, but to a very much less degree. Here the first symptoms were those of choroiditis.

"I need not detain you long in alluding to the vascularity of the eye that is described in connexion with 'aquo-capsulitis,' as it is the usual injection of the conjunctiva and sclerotica that exists in corneitis, varying, of course, in intensity in different cases."

**ART. 85.—*The use of Ice after operations upon the Eye.***  
By M. MAGNE.

(*Gaz. Méd. de Paris*, Sept. 22, 1855.)

In this paper several cases are related for the purpose of showing that consecutive inflammation may be altogether prevented in operations for cataract, &c., by applying ice to the eye immediately after the operation, and continuing the application for three or four days. The compress is soaked in iced water, and over this is placed a piece of ice enclosed in a small bag of some waterproof material, care being taken to prevent the ice from coming in immediate contact with the skin.

This idea is not new. On the contrary, it has been put in practice by M. Baudens, and also by MM. Chassaignac and Guernsent.

**ART. 86.—*Knife-needle for the operation for Cataract by solution or absorption.*** By Dr. ISAAC HAYS.

(*American Quarterly Journal of Medicine*, July, 1855.)

The operation for the removal of cataract by solution or absorption, has been deemed, by many surgeons, entirely inapplicable to hard cataracts, mainly in consequence of the difficulty of dividing such a lens by the needle ordinarily used for the purpose. The common straight needle cannot be made to cut well, beyond a short distance from the point, without being so thin as to endanger its breaking;

and it is not possible to *cut* with a curved needle. This difficulty, however, is met by a needle made somewhat after the fashion of an iris knife, and it is now more than three years since Dr. Hays has carried this idea into successful practice.

"This instrument, from the point to the bead near the handle, is six tenths of an inch, its cutting edge is nearly four tenths of an inch. The back is straight to near the point, where it is truncated, so as to make the point stronger, but at the same time leaving it very acute; and the edge of this truncated portion of the back is made to cut. The remainder of the back is simply rounded off. The cutting edge is perfectly straight, and is made to cut up to the part where the instrument becomes round. This portion requires to be carefully constructed, so that as the instrument enters the eye it shall fill up the incision, and thus prevent the escape of the aqueous humour. The handle should be octagonal, with equal sides, and of the same thickness its whole length.

"We have now used the knife-needle in a sufficient number of cases to be convinced of its superiority over any of the ordinary cataract needles.

ART. 87.—*A new operation for Ectropion.* By M. NÉLATON.

(*Lancet*, Oct. 6, 1855.)

The subject of this operation was a child affected with this deformity on each side, but the left upper lid was the worst. The eye was half open, and the cornea exposed; the free margin of the lid and the cilia were turned towards the eyebrow, and the tarsal cartilage was luxated in such a way that its superior border had become inferior. The cause of this state of things could not be learned, but it was suspected that a burn was the origin of it, as the skin was retracted and puckered. The external portion of the lid had escaped, but the destruction was complete towards the inner canthus, the free margin being only a few lines from the eyebrow.

M. Nélaton, considering that nodular retraction lasts only a certain time, judged that, by stretching the parts for the space of about one year, a cicatrix might be obtained which would have lost the tendency to contraction. He therefore made a horizontal incision in the groove which separates the eyebrow from the upper lid, and the latter, having thus been freed, was brought in contact with the lower lid. The margins of the upper and lower palpebræ were then pared, for the space of about four lines external to the punctum, and kept together by three stitches. In order to reduce the luxated tarsal cartilage, a strong needle was passed from below upwards through the lower lid and the cartilage, and the two parts were thus kept *in situ*. The stitches were removed three days after the operation, and the needle two days later.

The immediate results of the operation were satisfactory. The margins of the lids adhered completely on the internal half of the palpebral fissure, the external half being open, and allowing the

mucous secretion to escape. The upper lid was, in this external half, noticed to ride somewhat on the lower—a circumstance which was ascribed to œdema; and as granulations were suspected on the conjunctiva, M. Nélaton cauterized with the nitrate of silver. These proceedings were completed on the 4th of February, 1854, and the patient was then freed from all constraint, and expected to spend a year at least before an effort should be made to break up the union of the lids, when retraction might no longer be apprehended, and the ectropion be completely cured. Typhoid fever, however, carried off the patient two months after the operation, hence its results could not be ascertained. We are afraid the cornea would have been found somewhat injured by the protracted immobility, nor is it at all improbable that unpleasant adhesions might have formed.

ART. 88.—*On the cure of Myopia and Presbyopia by exercising the vision.* By M. JOBERT.

(*Archiv. Générales de Méd.*, Aug., 1855.)

In a memoir recently brought before the Academy of Science at Paris, M. Jobert maintains that a far-sighted or near-sighted person may acquire the perfect power of focusing the eye by mere practice. This, he tells us, is the result of his own personal experience. His theory is that the muscles which regulate the focal adjustment of the eye are unable to discharge their proper function in these cases, and that they acquire the tone necessary to do this by practice. He condemns altogether the use of spectacles, because spectacles render the mischief permanent by dispensing with the aid of the muscles which ought to be in action.

ART. 89.—*On the treatment of Strabismus.*  
By MR. CRITCHETT, Surgeon to the London Ophthalmic Hospital.

(*Lancet*, May 12 and May 19, 1855.)

The chief object of this paper is to set forth the advantages of a particular form of the subconjunctival operation. Speaking of the results of its adoption, Mr. Critchett says, “after having tried it in above a hundred cases, I never now adopt the old method, and I am strongly impressed with the uniformity of the favorable result; in no cases have I had increased prominence or eversion. In some cases, it is true, some amount of inversion has remained, but this occurred at least as frequently with the old operation; so that the patient is now sure of improvement from the operation, without risking the occurrence of any of the unfavorable concomitants of the old plan.”

The operation is described as follows:

Having placed the patient, if nervous or restless, or very young, under the influence of chloroform, the eyelids must be fixed open with a spring speculum (Fig. 1, *a*); the globe may be now everted by an



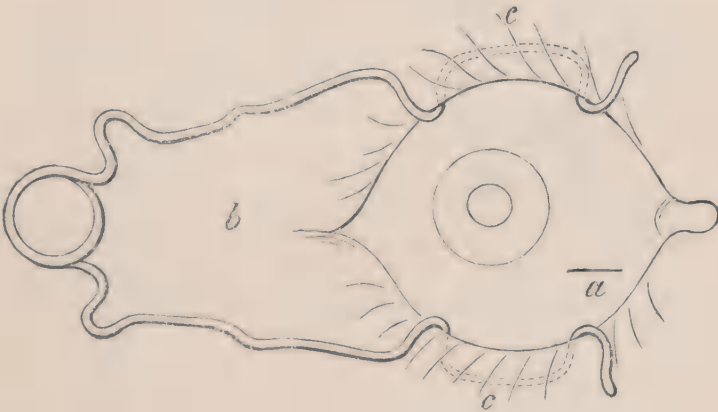
assistant, and the operator, seizing the conjunctiva at a point corresponding to the lower border of the internal rectus, makes a small

FIG. 1.



a. Speculum.

FIG. 2.



b, c. Speculum in action, the dotted lines representing it under the lids.  
d. Situation and size of incision in conjunctiva.

opening with a pair of rather strong blunt-pointed scissors (Fig. 3, g); he then seizes the subconjunctival fascia, and divides it to the same extent, so as clearly and cleanly to expose a small surface of sclerotic. The ordinary strabismus blunt hook (Fig. 4, e), bent at a right angle, must now be swept round the globe, so as to pass beneath the muscle; this may be known by the peculiar elastic resistance that it felt; the blades of the scissors must then be passed in through the opening, and by a succession of small cuts the tendon may be readily divided

between the hook and the insertion into the sclerotic, and close to the latter (Fig. 5, *f*). You may distinctly feel and sometimes hear the

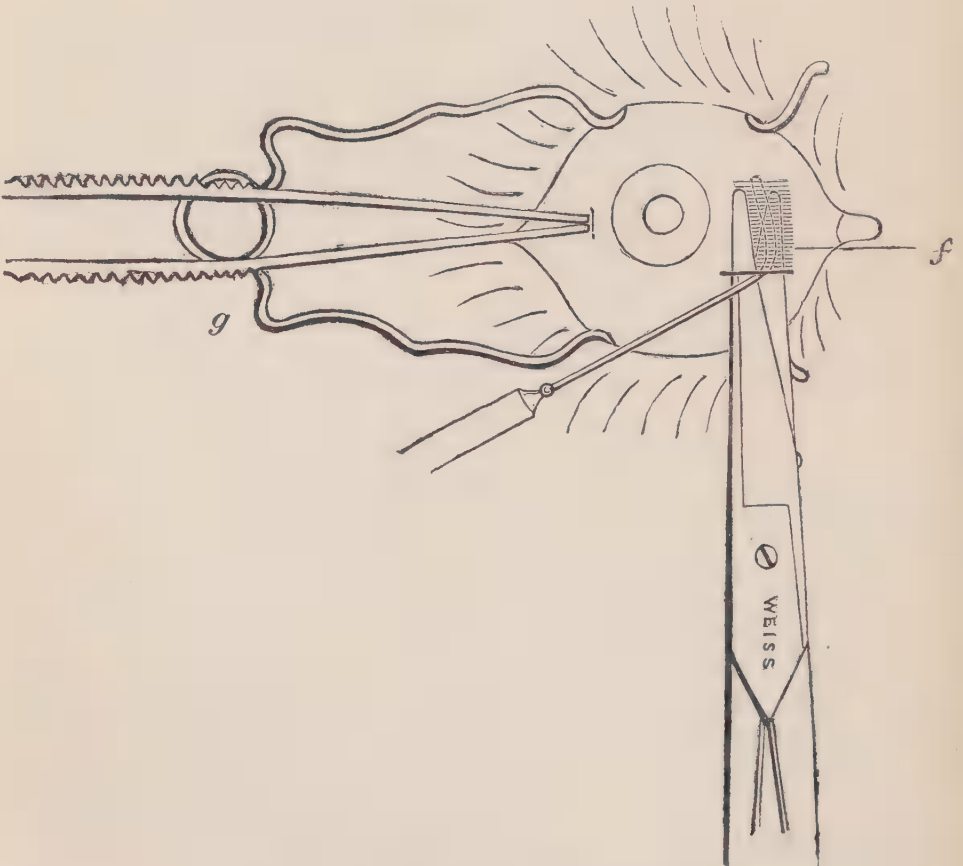
FIG. 3.

*g.* Strabismus scissors.

FIG. 4.

*e.* Strabismus hook.

FIG. 5.

*f* Hook and scissors in action under conjunctiva.

creak of the scissors as the tendon is cut through. Some little difficulty is sometimes experienced, when the insertion of the tendon is rather broad in reaching its upper edge, and when that is the case I make a small counter-opening in the conjunctiva corresponding to the upper border of the muscle. I introduce the hook from above, and, having passed it beneath the remaining slip of tendon, divide it with the scissors in the same direction. This counter-opening has the advantage of facilitating the escape of blood that has become infiltrated beneath the conjunctiva, and it does not in any way interfere with the principle and aim of the operation, which is to leave a broad band of conjunctiva between the cornea and the inner caruncle intact. The advantages of this plan, as contrasted with the old one, seem to me to be very great. It has, in the first place, the merit enjoyed by all subcutaneous sections, of immunity from inflammation and suppuration, and makes a very rapid and certain cure; no granulation ever forms, and the caruncle maintains its natural position, and does not shrink away into a deep fossa, as is invariably the case when the usual operation has been performed; and as far as my experience yet goes, proptosis or increased prominence of the eye is more rare, and eversion never occurs, and the natural movements of the eye are more complete. This I attribute to the fact that the ocular fascia is but little interfered with, and that a good firm union takes place between the divided muscle and the globe of the eye.

“Such seem to me to be the advantages of the mode of operating that I am now anxious to explain and recommend—advantages that are of so important a nature, that in fairly stating the case to the patient, if the old operation is contemplated, it certainly admits of doubt if the personal appearance is much improved, even in the most favorable results, and there is always a risk of increased prominence and of eversion, and it admits of a question whether it can be recommended. If, on the other hand, the mode of proceeding I am now setting forth be in contemplation, we may at least feel assured, that if the deformity is not altogether removed, it will not be rendered worse, and that in many cases the result will be so perfect, that the most experienced eye will not detect any defect, or be aware that any operation has been performed. But it may be asked if there are any objections to this operation, and any cases in which the old operation is preferable. It must be admitted that it is rather more difficult to perform, that there is a greater liability to leave some portion undivided, and that sometimes some inversion remains, in consequence of the attachment of the muscle to the fascia after it is divided from the sclerotic. This will often rectify itself afterwards, and where this is not the case, it is better either to operate on the other eye, or, if the cast is slight, be content to leave the case in that state, rather than risk eversion by further interference. It is only in cases of long standing, and where the strabismus is very extreme, and where the eye is small and deep-set, and where the subconjunctival operation produces but very little effect, that the old operation is justifiable.”



ART. 90.—*Double spontaneous dislocation of the Crystalline Lens.*  
By Dr. WILLIAMS.

(*American Quarterly Journal of Med. Science*, April, 1855.)

This curious case is taken from the Records of the Boston Society for Medical Improvement. There was no loss of transparency, though the accident was frequently repeated:—

CASE.—A female, æt. 30, of feeble constitution. At an early age she had measles, scarlatina, and variola, in immediate succession. Her sight has always been imperfect, and she has been able to see small objects—as in reading or sewing—only when held very near her eyes.

On the 8th of June last, she saw a circle—light in the centre, dark around its edges—which interfered with vision in her right eye; but no other change was observed till about a week before she was seen by Dr. Williams. At this time, in stooping to lift a tub, she was conscious that something occurred in her right eye, and soon experienced circumorbital pain and nausea. She afterward noticed a peculiar appearance in the anterior chamber. During the night these symptoms vanished; but vision was not as good the following day, as it had previously been. The same phenomena were repeated on subsequent days, after she had stooped forward.

When seen by Dr. W., on the 14th of November, in consultation with Dr. William H. Page, the appearances were as follows:—The right anterior chamber exhibited the crystalline lens in a perfectly transparent state, resembling, at first sight, a drop of oil between the cornea and the iris. Its weight caused the formation of a sort of pouch at the lower part of the iris, so that the lower edge of the lens was a line or more below the cornea, and its upper edge extended to rather above the middle of the pupil. The edge of the lens was sharply defined, as much so as that of a perfect cataract-glass of one inch focus. No attachment could be seen, nor did the aspect of the pupil indicate that any attachments passed through it. There was no injection of the eye. She had a constant sensation of nausea and discomfort, but less circumorbital pain than after the first prolapsus. Vision was almost abolished; but, on trial of cataract-glasses, it was at once improved, so that with those of two inches focus she was able to read.

As the sight of the other eye was very imperfect, this was also examined. It presented a very marked floating motion of the iris, similar to what is frequently observed after operations for the removal of the lens. Displacement of the crystalline was, therefore, presumed to have occurred in this eye also, and a trial with cataract-glasses proved the correctness of this supposition, as she could at once see perfectly. The lens was not visible in this eye; but there was an appearance at the lower part of the iris as if the lens were lying in this situation, and were impelled against the iris as the globe moved. Probably a dilatation of the pupil might have allowed the lens to be seen, but it was not thought advisable to expose the patient to a risk of a similar prolapsus to that existing in the other eye. Visiting her a few days after, when the right lens had fallen back into the posterior chamber, Dr. W. found it equally impossible to perceive any portion of the lens in this, as in the left eye. She required a glass of slightly greater power for the right eye; and, by the aid of these auxiliaries, enjoyed perfect vision.

On the 9th of December, the lens, which for several days had been in the posterior chamber, fell through the pupil, as before, and continued in the

anterior chamber for some time. If her head is held forward, in sewing, there is no displacement; but it is only in some unlucky moment of stooping, while engaged in active employment, that the lens is projected through the pupil.

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## SECT. II.—SPECIAL QUESTIONS IN SURGERY.

### (A) CONCERNING THE HEAD AND NECK.

ART. 91.—*A new method of treatment in Otorrhæa.*  
By Mr. YEARSLEY.

(*Lancet*, May 5, 1855.)

This new mode of treatment is neither more nor less than a modification of the remedy already introduced to the notice of the profession for the alleviation, if not for the cure, of all those cases of deafness that arise from partial or entire loss of the membrana tympani—namely, *cotton-wool*. Its influence, according to Mr. Yearsley, is not limited to the mere arrest and cure of the discharge; it has this additional superiority over the usual modes of treatment, that the sense of hearing, so frequently impaired under the use of astringents, is, on the contrary, not only not diminished, but decidedly, and in many cases immensely, improved. The treatment is to be carried into effect as follows:

“First of all, the passage of the ear is to be carefully cleansed by gently syringing it with warm water, and the moisture removed by means of a porte-sponge. The parts are now to be so clearly displayed by the aid of a powerful gas-reflector, that the necessary manipulations may be readily and accurately accomplished, when I take a small piece of dry cotton—the size of which varies according to the circumstances of the case—and adjust it by gently pressing down every part of it upon the surface from which the discharge proceeds, exactly as if dressing an ulcer on any other surface of the body; this done, quiet is enjoined, restricting, as much as possible, every movement of the jaw, such, for instance, as takes place in eating and speaking. Twenty-four hours afterwards I remove this, and apply another dressing of the cotton. The importance of restricting the patient from moving the jaws will be at once manifest, if the reader will take the trouble to place the point of a finger in the passage of the ear, and read aloud the present paragraph. It will then be perceived how easily the cotton, however accurately adjusted, may be loosened and moved from its state of exact apposition. In eating, this detachment takes place still more readily, yet the patient cannot be debarred all use of the jaw, seeing he must have food; nor, if great care be taken to keep the jaws in a state of motionless apposition, need speech be altogether interdicted; but for the same reason the food should be such as to require no mastication. Doubtless no one will consider these restrictions as objections to this mode of treatment; though a

more specious, but equally invalid objection to it may be raised, on the ground that the tympanum being a cavity, such a degree of accuracy in adapting the cotton to its surface, as described, cannot be attained. If the ear be examined with the admirable appliances for its illumination, now at the command of the aural surgeon, it will be found, in cases where the membrana tympani is destroyed, that the extent of the surface from which the discharge proceeds, is not only exposed to view, but the cavity is observed to be obliterated, and the walls of the tympanum, red and vascular, are seen thickened and tumid, if not spongy or fungoid. I speak here more especially of the worst cases that come under the notice of aural surgeons, in the great majority of which not only is the discharge itself cured, but the patient experiences a great amelioration in the state of his hearing also. Nay, more: cases can be referred to, in which the great disorganization of the ear seemed to preclude all hope of effecting any amelioration of the hearing, yet in which, after persevering in the treatment for a greater or less period, a change has been accomplished, which could not have been confined to the fungoid tissues alone, for, in the cases I speak of, a sensible improvement of hearing has been a coetaneous result."

Mr. Yearsley then relates four cases, of which we give two :

CASE 1.—Miss L——, pupil in the establishment of Miss Hurst, St. John's Wood, became my patient in June, 1854, suffering from a most disagreeable discharge from the right ear, which was left as one of the sequelæ of scarlatina several years ago. Considerable deafness attended the case, which varied with the state of the discharge, being greater when the latter was least abundant. On examination, a small perforation existed in the membrana tympani, below the insertion of the malleus; and the walls of the meatus, near to the membrane, presented a vascular appearance, approaching to a state of semi-ulceration. Contenting myself with cleansing the meatus, by carefully syringing it out with warm water, I directed her to apply a poultice, enclosed in a linen bag, to the side of the head, including the ear, for two nights in succession, and then to visit me again. At the second visit, the irritable appearance of the meatus had subsided, and I proceeded to adjust and impact a small piece of dry cotton at the bottom of the meatus. From day to day the same treatment was employed for upwards of a week, by which time all discharge had ceased. It was my wish to continue the application for three or four days longer, but arrangements had been made for her return to her friends for the holidays, which could not be overruled. As I feared, the result proved that the treatment was too early discontinued; for in six weeks she returned as bad as ever. This time she was instructed by her friends to attend me until the cure was complete; and this was happily effected in a period of three weeks. The discharge entirely ceased, and the hearing was perfectly restored. On examination of the membrane, no appearance of perforation remained.

CASE 2.—Mr. W——, surgeon in the navy, who had just received orders to hold himself in readiness to proceed to the Crimea, consulted me in December last for disease of the left ear, attended by a loathsome discharge. On examination, I discovered a small fleshy excrescence growing from the surface of the membrana tympani, which was very much disorganized, without any apparent perforation, though it seemed as if such a condition had at one time existed. The hearing was greatly deteriorated; but, having the sense perfect



on the opposite side, he was but slightly inconvenienced. The discharge, and a sense of oppression on the affected side, were the chief sources of complaint. I explained that it would be necessary first to remove the fleshy excrescence, and that then I should proceed to relieve him by my new mode of treating cases of otorrhœa, and that such treatment would require his daily attendance for several days in succession. At that time it was inconvenient to him to remain, and he returned to his duties at the Royal Naval Hospital at Deal, until he could make arrangements and obtain leave of absence for about a fortnight in town. In the interval he was one day exposed to a cold, piercing, easterly wind, and, wishing to protect the diseased ear, he pushed into the passage of it a piece of dry cotton, of which he took no further notice.

On the 10th of January, I received a note from him to say that he had obtained leave of absence, and would visit me on the following day, which he did. On examining the ear, I said, "Why, what have you got in your ear?" "Nothing; I have done nothing to it." "Oh yes, you have," I replied; at the same time withdrawing from the ear a dry piece of cotton, which had evidently been impacted there for several days. Again applying the speculum, I remarked: "The fleshy excrescence has disappeared, and you have unintentionally cured yourself of the discharge. You have absolutely cured yourself upon the principal of treatment of which I told you at your last visit. The piece of cotton I have just extracted has by some good luck been pushed down upon the seat of disease; its pressure has dissipated the excrescence, and with it the discharge has vanished." His astonishment was succeeded by an immoderate fit of laughter, which was thus accounted for: A surgeon had examined his ear the day previously, and said, "Oh yes, I see the fleshy growth quite plain; Mr. Yearsley will have no difficulty in removing it!" So much for the opinion of surgeons unaccustomed to see diseases of the ear. The gentleman alluded to could only have seen the pellet of cotton-wool. But more experienced surgeons than he may be deceived in regard to disease in the passage of the ear. I remember once to have removed a polypus from the ear of a young lady, the existence of which had been denied by two of the most eminent surgeons of the day.

ART. 92.—*On the significance of Ear-discharge.*

By Mr. TOYNBEE, Aural Surgeon to St. Mary's Hospital.

(*Medical Times and Gazette*, Aug. 18, 1855.)

"There can, I think, be no doubt that a discharge from the ear should always be regarded with suspicion. This fact is fully borne out by an inspection of the accompanying table, showing the relation between the duration of the discharge and the acute symptoms. The cases are taken from a paper which I published in the '*Medico-Chirurgical Transactions*' for 1851:

Disease in the base of the brain.		Disease in the cerebral cavity.		
Age of Patient.	Duration of Discharge.	Acute Symptoms causing Death, and their duration.	Post-mortem Appearances.	
42	35 years.	Pain in the head ending in coma; five days.	Pus in the tympanum and labyrinth and around the medulla oblongata.	
17	12 years.	Pain in the head and ear; twenty-two days.	Pus in the tympanum and labyrinth; auditory nerve of a dark colour; purulent matter deposited on the medulla oblongata, crura cerebri, and pons varolii.	
44	24 years.	Paralysis of the portio-dura nerve a few days before death.	Dura mater covering the upper wall of the tympanum thick and ulcerated; bone carious; mucous membrane of tympanum ulcerated.	
21	Occasionally for 14 years.	Violent pain in the ear and head; pain in the back and body; curvature of the neck backwards; delirium five weeks.	Tympanic cavity full of pus; a large abscess in right middle cerebral lobe.	
23	14 years.	Pain in the top of the head, followed by cerebral irritation; ten months.	An abscess in the left middle lobe of cerebrum; dura mater detached from the petrous bone; the bone soft and carious.	
10	5 years; also earache at times.	Headache, vomiting, chilliness, five days; was then convalescent; a day after intense pain in the ear came on, ending in death in five days.	An abscess as large as a small hen's egg in the left middle cerebral lobe; dura mater over tympanum very thick and ulcerated; carious orifice in upper wall of tympanum; tympanic cavity full of serofulous matter.	
Adult.	20 years.	Pain in the head for fourteen days; fever, coma, four days.	An abscess in the right middle cerebral lobe; dura mater ulcerated; upper wall of tympanum carious.	
24	3 years.	Cerebral irritation; abscess under the temporal muscle; delirium, coma; some days.	An abscess in the left middle cerebral lobe; the dura mater partly detached from the petrous bone, thick and dark coloured; the bone dark, but not carious.	
14	12 years.	Severe cerebral symptoms, coma, death in a few days.	A large abscess above the petrous bone, communicating with the external meatus, through petrous bone and dura mater.	

*Disease in the cerebellum and lateral sinus.*

60	2 years, followed by intense pain.	Violent cerebral irritation during thirteen days.	Dura mater covering the petrous bone detached from it, and full of orifices; an abscess in cerebrum; petrous bone carious; tympanic cavity and vestibule full of pus.
15	6 or 7 years.	Pain in right ear; shivering; headache; abscess behind the ear; great prostration; ten days.	Coats of lateral sinus thickened; coagulum in sinus.
45	20 years.	Pain in the left side of the head during the night only; cerebral irritation; delirium eight weeks.	The cavernous sinuses full of grey-coloured matter; mastoid portion of temporal bone carious.
27	Since early life.	Pain in the head, rigors, fever; an abscess over the mastoid process; stupor, coma, three weeks.	Lateral sinus full of pus; sulcus lateralis carious.
20	7 years.	Shivering, headache, and pain in the right ear, followed by abscess behind it; cerebral irritation; death in ten days.	Abscess in left lobe of cerebellum; sulcus lateralis carious; pus in lateral sinus; secondary abscesses in neck and right lung.
3½	2 years.	Pain in the ear and head; convulsions; great prostration; three weeks.	Caries of meatus externus and sulcus lateralis; pus in lateral sinus and jugular vein; abscess in neck; cerebellum soft.
9	At intervals for 5 years.	Pain in the ear and headache; abscess behind the ear; delirium; convulsions; five weeks.	Lateral sinus full of pus; sulcus lateralis carious, and its cavity continuous with that of the tympanum; purulent deposits in the lungs.
19	At intervals during 2 years.	Intense headache; tenderness of abdomen, great physical prostration.	Abscess occupying nearly the whole length of the right hemisphere of cerebellum; petrous bone carious and soft; tympanum full of pus; cerebrum healthy.
32	2 years.	Pain in the ear and side of the head; drowsiness, stupor, and coma; six weeks.	Abscess in right hemisphere of cerebellum; petrous bone carious; dura mater ulcerated.
Adult.	16 years.	Headache, stupor, coma; a few days.	Abscess in right hemisphere of cerebellum; external meatus and petrous bone carious.



"It is true that many persons live long, having had during the whole of life a discharge from an ear, without any disease of the bone; others live many years with a discharge, but at death the bone and dura mater are found affected, and might under many circumstances, have taken upon them an active state of disease, ending in the death of the patient. It is important, therefore, that you should be able to give an opinion respecting cases of the kind.

"In the first place it behoves you to decide from what source the discharge comes. If it arise from the dermoid meatus, and the membrana tympani is entire, there is, as I have before said, most probably irritation in the tympanic cavity or mastoid cells, of which irritation this discharge is but a symptom. Unless there were simply some eczematous state of the meatus to account for the discharge, and, unless the hearing power were perfect, such a case should be looked upon with suspicion, especially if it be attended by any symptoms of pain or cerebral irritation. Again, if the discharge issues from the tympanic cavity through a small or a valvular opening, and that it is requisite to blow the nose forcibly to clear out the tympanum, there probably is, or there will be, some affection of the bone, from the accumulation of the discharge. If there is a large orifice in the membrana tympani, or, if it is absent, if there is no ulceration of the mucous membrane of the tympanum, if there is some power of hearing remaining, and if by pressing and tapping the region around the ear, no pain is felt, and if there are no other symptoms of disease in the ear or head, I think you may assume that there is no disease of the bone, and that by attention to daily syringing, and the other plans alluded to when speaking of the treatment of these affections of the ear, there is a fair prospect of the affection remaining confined to the mucous membrane of the ear. On the other hand it is but fair for you to state, that negligence on the part of the patient, whereby the discharge would be allowed to collect so as to fill up the orifice in the membrana tympani,—a blow on the ear, an attack of fever, or any severe illness, might cause an irritation in the ear which, if neglected, might advance to the bone."

ART. 93.—*A peculiar serous discharge from the Ear after injury to the Head.* By Mr. HENRY GRAY.

(*Pathological Transactions*, vol. vi, 1855.)

The history and examination of this case appear to negative all the various theories which have been advanced in explanation of the origin of the serous discharge from the ear, as far, at least, as this case is individually concerned. A short epitome of these are subjoined.

Dr. Laugier, in 1835, supposed that the fluid was the serosity of the blood (extravasated between the bone and dura mater) which filtered through a chink in the os petrosum, passed into the cavity of the tympanum, and from thence into the external auditory canal.

That it was the serous secretion from the cavity of the arachnoid. —Guthrie, 1842.

Marjolin supposed that it was the liquor Cotugnii.

Chassaignac (1850) that it was the serum of the blood filtered through an abrasion of one of the venous sinuses in connection with the fractured temporal bone.

In 1850, Nelaton, Auguste Berard, and Robert, in France, Hilton and Prescott Hewett, in England, believed it to be the subarachnoid fluid escaping through a rupture of the arachnoid membrane and fracture, implicating the auditory canal and communicating with the tympanum.

That it is the saliva passing into the tympanum, through the Eustachian tube.

A man, æt. 45, who, up to the time of the present accident had enjoyed uninterrupted good health, was admitted into St. George's Hospital under Mr. Cutler, on the morning of the 18th of October, 1854, having fallen from a ladder twenty feet in height a short time before his admission. It was stated that he was stunned for a few minutes after the accident; he soon rallied, however, and his comrades, on picking him up, observed a bloody watery discharge flowing from the left ear. He walked into the hospital, where the attention of those present was soon called to this discharge. A small wound was found at the back part of the head, on the right side; he was quite sensible, and answered most questions readily.

On the 19th, he was still sensible, but he had no recollection of the accident; the discharge from the ear continued, and to such an amount, that two ounces was collected in less than an hour.

On the 20th, he became very restless, delirium came on, the pulse increased in frequency, but there was much less discharge from the ear. He was bled to nine ounces; but in the evening, although the pulse was softer, it did not diminish in frequency, the restlessness continued, and rather more serous discharge was poured from the ear.

On the 21st and 22d, diffuse inflammation of the areolar tissue of the scalp came on, which, notwithstanding the usual treatment, did not subside. He gradually sank, and died October 25th, seven days after the receipt of the injury.

The discharge, varying somewhat in quantity, continued from the time of the accident until the day previous to his death. Its quantity was so great that it saturated the pillowcase, and it became necessary to have napkins placed under the ear. It was repeatedly collected in gallipots, and was always found to be mixed with a minute quantity of blood. After being allowed to stand for a while, the blood-globules subsided to the bottom, forming a very thin layer of coagulum, the supernatant fluid still retaining a slightly roseate hue. On the day previous to his death it was mixed with pus. The discharge, when tested, was found to contain a large quantity of albumen; but no chemical analysis was undertaken, as its admixture with blood would have rendered the results fallacious.

On the post-mortem examination being made, a fracture was detected, commencing in the centre of the right cerebral fossa of the occipital bone, and just opposite to the wound already mentioned as situated in this region, it passed down through the corresponding cerebellar fossa, where it subdivided into two fissures, the innermost of which passed into the right margin of the foramen magnum, the outermost into the back part of the right jugular foramen. The triangular portion of bone included between these two fissures was comminuted. Another separate line of fracture commenced in the left margin of the foramen magnum, it passed obliquely outwards and forwards

through the groove for the lateral sinus, and terminated at the back part of the left jugular foramen, so that the fracture did not encroach upon the temporal bone. This bone being now removed, together with portions of the occipital and sphenoid bones, the dura mater and other soft parts were detached from its various surfaces, and the bone was minutely examined, but no fracture could be detected in any part. This examination was carefully repeated, and at different times, but still no lesion of the bone could be discovered. The internal auditory canal was now examined; the tube of the arachnoid membrane accompanying the seventh pair of nerves was quite normal, but a minute quantity of blood was found in the subarachnoidean tissue surrounding the nerves. The cochlea, vestibule, and semicircular canals were then examined; they were healthy. On laying open the tympanum, its cavity was full of a thick tenacious muco-purulent fluid, and a similar secretion was found at the tympanic orifice of the Eustachian tube, and also in the mastoid cells. On washing this away, the lining membrane of the tympanum was found to be intensely vascular. In the Eustachian tube this vascularity ceased at the tympanic orifice, but the membrane lining the mastoid cells was as vascular as that lining the tympanum. The ossicula were healthy, and presented their usual arrangement; the stapes was firmly lodged in the fenestra ovalis; the fenestra rotunda was covered by its peculiar membrane. There was consequently no communication between the internal ear and the tympanum. The membrana tympani was ruptured; the aperture, about the size of a small pea, was situated at its anterior and inferior angle.

ART. 94.—*The effects of accumulations of Cerumen.*

By Mr. TOYNBEE, F.R.S.

(*Pathological Transactions*, vol. vi, 1855.)

These effects are sometimes serious. They may be enumerated as follows :

1. Simple dilatation of the meatus.
2. Absorption of the posterior wall, so as to allow of a communication between the cavity of the meatus and the mastoid cells.
3. Absorption of the anterior wall, so as to cause an orifice communicating with the fossa parotidea.
4. Absorption of the superior wall, producing an aperture into the tympanic cavity.
5. Pressure upon the outer surface of the membrana tympani, rendering it extremely concave.
6. Inflammation and thickening of the membrana tympani.
7. Perforation of the membrana tympani.
8. Perforation of the membrana tympani, and protrusion of the cerumen into the tympanic cavity through the orifice.

ART. 95.—*A new artificial Membrana Tympani.*

By Mr. THOMAS WESTROPP.

(*Assoc. Med. Journ.*, Oct. 12, 1855.)

This contrivance is thus described :—"In the first place, my contrivance is a tube, very thin in texture, very pliant and durable in



material, with a flat vibrating membrane at one end, the other extremity being open to admit the entrance of sonorous undulations. It is thus made: having accurately inspected the meatus, into which we desire to insert an artificial membrane, we must make a model of it in some hard timber (a cast is out of the question,—I frequently failed in the attempt); this model should be almost as perfect as a cast itself, though not too tight for the meatus; its end should be rather flat, and the circular edge nicely rounded off; the whole should be smooth and polished; this model, or, if we like to term it, timber cast, previously oiled, should be repeatedly dipped into a thin solution of gutta percha in chloroform till a film of sufficient thickness be formed to peel off in one unbroken piece; if the tympanal end of the timber model be of greater diameter than its centre (after the manner of the meatus itself, but this is not absolutely essential), it will be necessary to make a small slit in the side with a knife, but the incision should not approach within a quarter of an inch of the extremity, where the flat membranous part lies. This tube, if found of unequal thickness in any position, a slight coating of the solution may be applied so as to remedy the deficiency; if the timber cast, or model, has been properly made, and all successive steps accurately carried out, the membrane, when cut with a pair of scissors to the required length, will be found to fit the meatus pretty comfortably, and when oiled and coated with cerumen, to exclude the external air from the *cavitas tympani*. The tube itself should not be allowed to protrude, but should be cut obliquely, so as to lie entirely within the meatus. It easily adapts itself to the parts; the flat end lies at the proper angle in the site of the lost membrane, or on its remains. When it becomes advisable to clean its surface, it can be easily taken out by the patient with a small tweezers, washed, oiled, and reinserted; if found to fit, two or three of the same size should be made, and given to the patient, who should be taught how to use them.

“In conclusion, I must remark that though my contrivance for supplying an artificial *membrana tympani* is simple, still I do not expect that every person who tries to make or adjust them will succeed at first; it requires much practice. They will as often fail as succeed in the attempt to construct a perfect membrane, as it is a difficult matter to hit upon the proper thickness of the membranous tube. The solution of gutta-percha requires to have a certain consistency and no more; it should be rather thin, so as not to coat the timber model irregularly, and to allow of its spreading evenly over its surface; each coating should be allowed to dry perfectly: this must be repeated six or eight times during a space of two or three days. The tube should not be taken off when too thin in its substance, or it will tear; it should not be made too thick, or it will be hard, tough, and irritate the meatus; but it should be about as thick as very fine sheet gutta-percha, or oil-silk: it then is pliable, soft to the ear, and will easily vibrate when adjusted; in short, the thinner it is made consistently with durability, the better.”

## (B) CONCERNING THE CHEST, ABDOMEN, AND PELVIS.

ART. 96.—*A curious case.*

By Mr. WILLIAM COLLES, Surgeon to Steevens' Hospital.

*(Dublin Quarterly Jour. of Medicine, May, 1855.)*

The peculiarity of this case is, that death was occasioned by a fish-bone (the pre-operculum of a herring) sticking in the throat and piercing the aorta through the œsophagus.

CASE.—John Bryan, æt. 56, a labourer, was admitted into Steevens' Hospital, March 30, 1855. About three or four o'clock the day previous to his admission, while eating his dinner, he swallowed a fish-bone, which, he states, he felt cutting him very much at the time "in his chest" (his own words), the cutting pain being increased exceedingly by the act of swallowing. Immediately after, he commenced to spit up large quantities of blood of a dark colour, which, however, soon changed its character, being bright red. He did not apply for admission into hospital till twelve o'clock the next day. He then complained of acute pain in his chest, and of great weakness. He had a blanched appearance, and a decidedly hemorrhagic pulse. Immediately after his admission he vomited up a fishbone about an inch long, of a very irregular shape, having a number of sharp points, and cutting edges. He continued to vomit up a good deal of blood throughout the day, but not so much as at first; the quantity gradually diminishing until nine o'clock the same evening, when he died.

*Post-mortem appearances.*—On opening the thorax, about three ounces of reddish-coloured serum were found in each pleural cavity, and about an ounce of fluid of a similar appearance in the pericardium. The posterior mediastinum was filled with coagulated blood. Upon removing the œsophagus and slitting it up, there was seen upon its posterior wall an oblong irregular opening, about half an inch in length from above downwards; the opening corresponding with the termination of the descending portion of the arch of the aorta, through which there was a slit, or tear, exactly opposite to that in the œsophagus, but differing from it in being smaller and more irregular. Upon examining the abdomen, a large clot of blood was found in the stomach, and the small intestines were filled with a similar fluid.

ART. 97.—*Gunshot wound of the Heart.* By Dr. CARNOCHAN, Surgeon-in-Chief to the State Hospital, New York.*(American Medical Monthly, April, 1855.)*

This case is one to be added to the few already on authentic record showing that penetrating wounds of the heart are not always immediately mortal. It has, moreover, peculiar features which will render it remarkable in the annals of surgical pathology. Several cases are mentioned in which patients have survived one or more days the effects of penetrating and non-penetrating wounds of the heart, inflicted by cutting instruments, and also of non-penetrating wounds inflicted by gunshot. But the peculiarity of this case is, that although the wound was a penetrating gunshot wound, leaving the ball deeply

buried in the tissue of the heart, the patient survived for a period of time so long as to encourage the hope of recovery. This position of the ball distinguishes the case from that mentioned by the French surgeon, Latour, where the ball had not penetrated deeply into the heart, but rested on its surface, partially encroaching upon the muscular wall, and being enfolded partly by the pericardium. The autopsy of this case also revealed that the wound was not only closed and cicatrized, but that a cyst was in process of formation around the ball. By this case, also, it is established that hemorrhage is not necessarily a consequence of a gunshot wound of the heart; for the serum found in the pericardium was merely tinged with blood, and there was no coagulum. The absence of hemorrhage may be accounted for by the conical shape of the ball, and by its direction; two circumstances which favoured its passage between the muscular fibres of the superficial layer of heart, without severing them, and caused it to rest slantingly behind the anterior coronary artery, without wounding it.

On the 27th of February, 1855, I was called in consultation to see William Poole, a young man, æt. 33, of unusually athletic form and muscular development, who had been wounded two days previously in an affray with firearms. He had received a bullet wound in the outer aspect of the right thigh, two inches above the upper border of the patella. The wound, however, which created alarm among his friends, was situated upon the anterior wall of the thorax, about three quarters of an inch to the left of the mesial line, and about half an inch below a line drawn across the chest, from one nipple to the other. A bullet probe could be passed slantingly from right to left, along the track of the wound, for about an inch. At this depth the probe was arrested, and it was not thought expedient to use force in making further exploration. Poole received his wounds during a deliberate onslaught made on him by some five or six persons armed with Colt's revolvers. The first ball took effect on the right thigh, and brought him to the ground. While thus prostrate, another assailant placed the muzzle of a pistol close to his chest, and discharged its contents. He immediately jumped up, and reeling towards a door, rested, as if stunned, against it for support, during some minutes. He then fell, exclaiming that he was dying, and remained senseless, cold, almost pulseless, and apparently moribund, for about four hours. From this condition he rallied, and became so free from the usual symptoms of severe injury, that his medical adviser, Dr. Putnam, considered that the ball had really not penetrated into the thoracic cavity, and my opinion was sought to corroborate or dispel this favorable view of the case.

I found him sitting in bed, his back resting on pillows as a support, apparently at ease, and conversing with numerous acquaintances, who had come to visit him. His countenance exhibited no expression of anxiety, and he answered placidly and without effort the questions I put to him. His pulse was 80 in the minute, the respiration easy, the surface of the body normal in temperature and moist. The stethoscope revealed the existence of no difficulty in the respiratory passages, and the normal *tic tac* of the heart beat with healthy precision. There were no signs of inflammation or of effusion into the pericardium.

With such freedom from morbid symptoms, I was disposed to concur with his medical adviser in auguring favorably of the case; for although it might be inferred, from the external character of the wound, that the ball had



passed somewhere into the cavity of the chest, it was not impossible that it had become lodged in some position where it remained innocuous.

The previous treatment had been gently antiphlogistic; mild aperients, diaphoretics, acidulated drinks, and low diet. The consultation resulted in a continuation of a similar mode of treatment, with the injunction that he should be kept in a state of absolute bodily rest, and free from every cause of mental excitement, as I felt far from certain that he had not sustained mortal injury.

The symptoms in Poole's case illustrate in a remarkable degree some of the peculiarities of wounds of the heart, and also the assertion made by Harvey, that the heart is not very sensible. I am informed by Dr. Putnam, who saw him at one o'clock on the morning of the 25th, about fifteen minutes after the wound was received, that the patient was at first nearly pulseless, was insensible, and that respiration was performed with great difficulty. In this condition labouring also under the ordinary signs of shock to the system from a gunshot wound, he continued for about four hours, before any signs of reaction were manifested. Vomiting now occurred; this was followed by increased action of the heart, and sensibility gradually returned.

During the same day (25th) he continued improving; and on the evening visit, the pulse beat 84. The skin was moist and natural, tongue healthy, with no unfavorable symptoms otherwise. No external hemorrhage had occurred from the wound, nor had any evidences of internal hemorrhage been evinced by vomiting or expectoration.

26th.—The wound was examined more particularly, and no traces of the bullet could be found, nor any special indications manifested of its presence in the cavity of the thorax. Symptoms about the same.

27th.—I saw the patient for the first time, and found him in the favorable condition already stated.

28th.—Complained of slight headache; pulse 86; bowels not having been moved, a gentle aperient was ordered, by which the pain in the head was relieved. At times the patient had complained of transient and slight pain about the region of the heart.

March 1st.—Was called in to see patient a second time. Had slept well; pulse 80; respiration natural; appetite good; skin moist; action of the heart natural. He stated that he felt no pain or unpleasant symptom, except weakness, remarking, however, that he felt well enough to go out.

2d.—The patient perfectly comfortable; pulse 82.

3d.—Patient so well that, upon visiting him, for the third time, by request, he was found receiving his friends, and, contrary to previous injunctions, conversing freely with them. Enjoined repose.

4th.—No untoward sign connected with either the functions of circulation or respiration. During the day, he received, against positive orders, the visits of more than a hundred people, with whom he conversed. His own statement was that he felt quite well.

5th.—Dr. Putnam was sent for early in the morning. At 8 o'clock a.m., the patient was found in a high state of irritability; pulse 120; skin hot and dry, and complains of pain generally; respiration troubled and more frequent. An aperient was ordered, by which the symptoms were much alleviated.

6th.—Was again requested to see the patient. Pulse 100; countenance anxious; the adnata tinged yellow; complained of debility, but said he had no pain about the heart; signs of effusion.

7th.—Passed a restless night, notwithstanding the administration of an anodyne; pulse 120; countenance more anxious; respiration much troubled;

nability to remain in the recumbent posture; symptoms gradually becoming more grave.

At 2 a.m., Thursday morning, his attending physician was sent for. The patient was now rapidly sinking; pulse almost imperceptible, and with difficulty counted; respiration short, frequent, and difficult; extremities cold; countenance pallid and hippocratic. From this time he continued to sink, and expired, without a struggle, at five o'clock.

*Autopsy seven hours after death.* The body was in a state of perfect preservation, and showed a powerful and well-developed organization. The surface of the body presented three orifices of gunshot wounds: two on the external side of the right thigh, a short distance above the patella, by which, apparently, a ball had made its entrance and exit respectively; and one on the anterior aspect of the chest, three quarters of an inch to the left of the median line, and about half an inch below a line drawn across the chest, from one nipple to the other. The examination revealed that all the organs of the body were in a healthy condition. The sternum and cartilages of the ribs having been partially elevated, a bullet-probe could be passed without difficulty, slanting from right to left, through the wall of the thorax, at the place of junction of the cartilages of the fifth and sixth ribs with the margin of the sternum. The sternum being completely elevated, the pericardium was seen to be much distended, and on its surface, in continuation with the external wound, was observed a rough spot, which proved to be an opening into the cavity of the pericardium, thinly closed by the exudation of plastic material. The right and left cavities of the pleura were free from effusion, and the lung on each side was in a sound condition. The pericardium was found filled with serous fluid, tinged with blood, and was so distended that it encroached very much upon the lungs on both sides. Upon opening the sac of the pericardium, and removing the large quantity of serous fluid, the external surface of the heart and the serous lining of the pericardium were both found to be entirely covered with plastic exudation, presenting all over signs of high inflammatory action. A cursory examination of the heart in position did not disclose the presence of any foreign body. It was afterwards taken out, and, upon a careful examination, a bullet, one inch in circumference, was found enveloped in a delicate cyst, and imbedded, to the depth of a quarter of an inch, in the muscular tissue of the septum, between the right and left ventricles, about midway between the apex of the heart and the base of the ventricles. Its locality was only indicated by the sense of touch, for as the wound had entirely cicatrized, there was no outward visible sign of its presence. Obviously, the cause of death was inflammation of the pericardium and heart, and its results.

#### ART. 98.—*On wounds of the Heart.* By Dr. PURPLE.

(*New York Journal of Medicine*, May, 1855.)

The following conclusions are deduced from a very elaborate and careful paper containing the particulars of forty-two recorded cases of wounds of the heart:

“That wounds of the heart are not in general immediately fatal.

“That recovery after severe gunshot, incised, and punctured wounds of the heart is possible, and that, too, amounting almost to a probability, provided a careful and judicious treatment is faithfully carried out.

“That the presence of a leaden ball imbedded in the walls of a ventricle of the heart does not preclude the possibility of recovery, and is not incompatible with the continuance of life for a number of years.

“That it is possible for an incised wound of the heart to heal by first intention, and the patient afterward be able to continue a laborious occupation for years after with no severe manifestations of heart disease.

“That the presence of a foreign body, other than a leaden ball, of considerable size, in the walls or cavities of the heart, does not necessarily preclude the possibility of a continuance of life for a number of days.

“That the *prognosis* of all wounds of the heart is unfavorable, but that in some cases hopes of recovery may be entertained, provided the patient's constitution be good, and efficient treatment be early resorted to.

“That the proper treatment of wounds of the heart is that which is adapted to like wounds of the chest in general; and that the inflammatory complications must be met with the same remedies as are adapted to the management of the disease when arising from idiopathic causes.

“That all parts of the heart are not equally liable to wounds, the right ventricle being the one most frequently injured.

“That the comparative mortality of heart wounds shows that the average duration of life is greater if the left ventricle be the seat of injury. This proposition is opposed to the received opinion of almost all writers on this subject.

“That the medico-legal relations of wounds of the heart are important, and should command the surgeon's careful attention, in order that he may not jeopard the life of his patient by timidity on the one hand or temerity on the other; and thereby subject himself to the inconvenience of the raising of a false issue on the management of the case before a legal tribunal.”

ART. 99.—*Ruptured Intestine after the application of the Taxis.* By Mr. NATHANIEL WARD, Assistant-Surgeon to the London Hospital.

(*Pathological Transactions*, vol. vi, 1855.)

This case is particularly interesting as showing the extreme caution that should be used in the application of the taxis, in cases of recent femoral hernia, in which the symptoms of strangulation have existed even for a few hours. It would have been much better, in fact, had it not been had recourse to at all in this case, as an immediate operation without it would possibly have saved the life of the patient.

CASE.—A labourer, æt. 56, was admitted into the London Hospital, suffering from symptoms of strangulated hernia. They had existed for forty-seven hours; and about five hours before admission the taxis had been used, with-



out any undue amount of force, by a medical man. The result of its application was the sensible diminution of the tumour, without relief to the symptoms of obstruction. At the time of his admission there was considerable fulness in the right femoral region, and Mr. Gowland deemed it expedient to make an exploratory operation. Different layers were cut through, until, on opening what appeared to be the sac, a quantity of yellowish fluid, mixed with air-bubbles, escaped. The patient died forty-eight hours after admission, peritonitis having supervened on the symptoms of strangulation.

*A post-mortem examination was made fourteen hours and a half after death.*—Evidence of acute serous inflammation existed. About an inch to the pubic side of the femoral ring, and not more than twelve or fourteen inches from the duodenum, was the portion of gut that had originally constituted the hernial protrusion. It represented about the lower two thirds of the calibre of the intestine, and had the appearance of a prominent pouting excrescence, with a large aperture, through which a sixpenny piece could have been passed into the interior of the intestine, and springing, apparently, from a constricted neck, which was the part which had evidently been girted round by the femoral ring. The walls of the apparent excrescence stood firmly out in consequence of the inflammatory exudation that had taken place in and between its coats. The borders of the aperture were thin, ragged, and sloughy, and at its back was another small irregular aperture, with a sloughy border.

This patient had never previously been subject to rupture, and he attributed its occurrence to having received a heavy blow from a quantity of earth that fell on his back.

#### ART. 100.—*On Strangulation in empty Hernial Sacs.*

By M. CHASSAIGNAC, Surgeon to the Hospital Lariboisière.

(*Rév. Méd.-Chir. de Paris*, May, 1855.)

The evidence collected in this paper is sufficient to show that strangulation in empty hernial sacs may give rise to all the symptoms of strangulated hernia. These symptoms are much more severe when the sac communicates with the peritoneum—sometimes more severe than those of strangulated hernia. The sac, under these circumstances, may contain serum, pus, or blood. It collapses when punctured (and thus the absence of bowel or omentum may be detected), but it cannot always be emptied by the taxis, even when there is a communication with the peritoneum.

All the cases, as yet, have been in females.

Excision of the sac is recommended as the best mode of treatment.

**CASE.**—A woman, æt. 52, had long suffered from irreducible femoral hernia on the left side, and for which she had worn a badly-fitting truss. Symptoms of strangulation had been present from the previous evening, and signs of severe peritonitis were unquestionably present. The tumour was as large as a hen's egg, and extremely tense and tender.

M. Chassaignac operated, on the 14th of September, 1854. He divided several layers before he got to the sac, and when he had done this, he could find nothing in the sac but fluid. On puncturing with a needle, the sac completely collapsed. No communication with the peritoneum could be found when a larger opening was made.

The treatment pursued was to excise the sac, and dress the wound in the ordinary way, but without relief to the symptoms. On the contrary, the stercoraceous vomiting continued, and the patient sank in the course of the next day, apparently from general peritonitis. There was no autopsy.

Some other cases are related, but this will serve as an illustration.

ART. 101.—*Acute Orchitis treated by the local application of Ice.*  
By Mr. CURLING, Surgeon to the London Hospital.

(*Medical Times and Gazette*, March 3, 1855.)

Cold lotions and ice have been frequently resorted to as subsidiary measures in the treatment of this affection, but we do not know that any surgeon has depended exclusively upon the local application of cold.

CASE 1.—*Acute Orchitis from a Blow (?)*.—*Six Days' Employment of Ice.*—*Cure.*—Thomas H—, a slight, delicate-looking lad, æt. 18, admitted on January 30, 1855, on account of swollen testicle. On examination, the right testicle was found much swollen, hot and red, hard and tender. It formed a nearly uniform tumour, of from twice to three times the size of the healthy gland. The body of the testis seemed to be the part inflamed, rather than the epididymis. The boy's statement was, that it arose from a fall, with his legs separated, on the previous day; and to this statement he pertinaciously adhered. There was evident redness, however, of the tips of the urethra; and a small quantity of thin, opaque, white discharge was made to appear on squeezing out the canal. He was in considerable pain, which increased on attempting to walk, so that he could hardly get from one room to the other. He was put to bed; the testicle was supported by means of a crutch-pad applied transversely beneath it; the piece of bandage attached to each end of the pad was brought above the crest of the ilium, and secured around the body. Ice was applied in a bladder to the testis; and the cold was carefully maintained by using large fragments of ice, and by putting in fresh pieces as often as liquefaction took place.

*Vespere.*—The pain is greatly relieved; the scrotum firmly contracted; the testicle cold, and the skin covering it blanched. Tenderness is much diminished.

January 31.—He suffers but little pain; the heat and tenderness of the testicle have much diminished. Continue the ice. To take a purgative powder of calomel and jalap.

February 2.—There is now little or no pain in the testis; no excess of heat; and the tenderness is much diminished. It is decidedly softer and smaller than when he came in.

5th.—The size and tenderness of the testis have steadily diminished day by day. It now scarcely exceeds the other in bulk. Allowed to get up, and to discontinue the ice.

8th.—The testis still slightly exceeds the other in size, but feels soft and loose, and free from tenderness. Ordered a suspensory bandage, and discharged cured.

In this case the ice was used without intermission, night and day, from January 30 to February 5. With the exception of a purge, no other remedy was resorted to.

From the circumstance, that, in the above case, the gland itself, rather than the epididymis, was the part affected, Mr. Curling was inclined to

think it not improbable that the lad's statement as to the cause of the disease was correct, and that it had really followed an injury. If so, its speedy cure is yet the more satisfactory, since orchitis after blows or of idiopathic occurrence is usually much more intractable than the gonorrhœal form.

In the next case, the man's statement as to a blow was probably false. No doubt the disease was gonorrhœal.

CASE 2.—*Acute Orchitis after Gonorrhœa.*—Three days' employment of *Ice.*—Cure.—H. H—, æt. 25, was admitted on account of swollen testicle, on February 2, 1855. He stated, that in getting out of a van he had fallen across a wheel, the edge of the wheel striking the testicle. The accident was said to have occurred on the day previously. The right testis was red, hot, and tender and swollen, so as to form a tumour about three times the size of the healthy gland. The hard, swollen, and very tender epididymis could be felt distinctly at the outer and back part; the rest of the tumour was not so hard as in the previous case; and, from the obscure fluctuation in some spots, conveyed the impression of partial effusion within the tunica vaginalis. On the inferior aspect of the penis, just behind the corona glandis, there was an unnatural opening, through which the urine was accustomed to flow. The natural urethral orifice was also present. A little whitish discharge was present at the abnormal orifice; he admits that he has had a discharge for about a fortnight; at first thin and transparent, afterwards white and opaque. He was put to bed; a crutch pad was applied, and ice used in the same way as in the former case. Calomel with jalap, ʒj, was administered, and milk diet and beef tea ordered.

*Vespere.*—He feels much easier.

February 3d.—There is very little pain in the testis, and the tenderness is much diminished; the heat and redness are kept down by the cold application.

5th.—He has steadily improved since the last date. The testis has gradually decreased in size, and become less tender and painful. Complaining of dampness and discomfort produced by the ice, he was allowed to discontinue its use by night. Middle diet.

8th.—The testis is now very little larger than the other gland. The heat, pains, and tenderness, are quite gone. Discharged cured.

In this case, as in the former, with the exception of a purge, ice was the sole remedy employed; and from February 2d to February 5th, its use was maintained day and night with the same regularity.

In some clinical comments on the above cases, Mr. Curling directed attention to the following recommendations of this plan of treatment:—1st. Its efficacy; both of the cases having yielded quickly, and perfectly. 2d. The early and efficient relief to the pain afforded by the benumbing influence of the cold. 3d. The regular and even compression of the inflamed gland procured by the cold inducing tonic contraction of the dartos. 4th. The saving of the patient's strength by the avoidance of all depletory measures.

ART. 102.—*On Lithotrity.* By Mr. SYME, Professor of Clinical Surgery in the University of Edinburgh.

(*Lancet*, May 26, 1855.)

“My opinion regarding lithotrity is, that, while some patients will get off more easily by it than by cutting, yet, on the whole, it is less



satisfactory. Take twelve patients with stones suitable for crushing, and treat them all in this way; some will, perhaps, get off without any further trouble; but of the remainder some will suffer from irritability of the bladder; some will have a return of the symptoms, in consequence of fragments having been left to form the nuclei for subsequent concretions; while others will be set free from further trouble by death occurring within a few days after the operation. You must also take into account that there is much more practice required, in order to perform lithotrity properly, than is necessary for lithotomy, in which ordinary surgical skill is sufficient, and that it is only in the hands of the most expert operators that lithotrity has even the degree of safety which I have admitted, and that otherwise it is far more dangerous than lithotomy, so that every man who aspires at learning this art must lay his account with a great deal of discomfort in his early cases. On the whole, therefore, I am of opinion that the wholesome, effectual, and, I will add also, safe method of excision should in general be preferred to crushing."

ART. 103.—*On the uselessness of "Tunnelling" in the treatment of Stricture.* By KELBURNE KING.

(*Edin. Medical Journal*, Oct., 1855.)

We take these remarks from the account of a case of supposed impermeable stricture of the urethra which was cured by dilatation.

"In surgical writings and lectures," says Dr. King, "we occasionally find it recommended to make pressure against a stricture which does not readily yield. In this way it is said absorption is promoted, the strictured part gradually reduced, and the ultimate cure greatly facilitated before the stricture has been permeated at all. This process has received the name of "tunnelling," and has been recommended on high authority. Now, there are doubtless many strictures which do not admit of penetration at the first attempt, and it may soothe a patient's mind to allow him to suppose that these unsuccessful attempts clear the way for what is to come after; but I have never observed, in my own experience, that any real good has followed from them. In the case related, it was tried for years, but without even retarding or alleviating the progress of the symptoms. However it may be explained, I have never seen any beneficial result until an instrument can be fairly passed into the bladder. I would, therefore, advise no one to linger tunnelling on the threshold of a stricture, but, with all expedition, by patient, steady, and gentle manipulation, strive to penetrate it, regarding that as the first indispensable step towards a cure."

ART. 104. *On catheterism in cases of confirmed Stricture.* By Mr. HENRY THOMPSON, Surgeon to the St. Mary-le-bone Infirmary.

(*Lancet*, June 23, 1855.)

"There is a circumstance very necessary to be remembered by the operator in relation to catheterism in cases of confirmed stricture,

which does not appear always to have received that marked attention which its importance demands. I have had several opportunities of exhibiting examples of the condition referred to at the Pathological and Medical Societies during the past year. It is well known that posterior to an old organic constriction of the urethra a considerable degree of dilatation often exists. The canal behind is gradually extended and opened out by the hydrostatic pressure occasioned in the habitual and powerful straining to pass urine which the patient is compelled to exert. Not only, however, does dilatation occur, but frequently also a fasciculated condition of the prostatic urethral walls, very much like that which we see in hypertrophied and fasciculated bladders, so that numerous fibrous bands intersecting each other appear prominent beneath the mucous lining, and interstices of corresponding depth and magnitude are seen between. It is not difficult, in examining these, to comprehend how that the difficulty of the case is by no means surmounted, when the point of the instrument has been insinuated, after much trouble, through the narrow channel of the stricture. We should not then at once push on our acquired success; for here indeed is a source of danger, greater in some cases than any that has before been encountered. Nothing is easier than to entangle the point of a small instrument in the meshes of these fibrous bands, and nothing is calculated to be more mischievous than any laceration in this posterior part of the canal. We cannot be too careful in the management of the instrument after the stricture has been surmounted, not only on account of the possibility of the existence of the difficulty described, but also because the delicate appreciation of an obstacle is far less easy after the catheter has passed through the stricture, and has become embraced by it, since it is in some degree difficult to recognize a slight degree of resistance which may offer itself beyond the point at which the grasp of the constriction interferes with freedom of motion on the part of the instrument.

"A preparation recently added to my collection was taken from a case, the particulars of which form an apt illustration of these remarks. It belonged to a patient whom I was requested to see when labouring under complete retention. Attempts had been made to relieve him, both in the hot-bath and out, but without success. I found him almost comatose, and with great depression of the powers of life. He was seventy-three years of age, and had suffered from stricture for many years. The condition of retention had been discovered about twenty-four hours before, but it had probably existed to a greater or less extent for a much longer period than this. Passing a No. 8 catheter, I found the obstruction distinctly marked in the bulbous portion of the urethra. With tolerable ease a No. 1 was carried through it, and here I encountered the obstacle, which, from what I learned, was probably that which had previously presented the chief difficulty. The instrument was felt in the rectum with more than ordinary distinctness, suggesting that it might be beneath the prostate gland, and lying in a false passage. A degree of mobility, however, and the fact that the stricture was one of very long standing, suggested that the point of the instrument was involved in the meshes of a dilated prostatic urethra behind the organic constriction. Ac-

cordingly, with a little withdrawing and manœuvring of the point, I was enabled to carry it into the bladder, drawing off about thirty ounces of dark and ammoniacal urine. I then tied it in the usual way, with the bent tube attached. The patient dying in a day or two after, from a degree of exhaustion which his age and general infirmities did not permit him to overcome, I found, on careful examination of the specimen, that there was no false passage, but just the condition which has been alluded to. The atrophy, or thinning of the prostate gland from dilatation, especially at its inferior part, accounted for the ease with which the instrument could be felt by the finger in the rectum, and the recesses and dilated orifices seen there had, without doubt, occasioned the difficulty in carrying the catheter into the bladder. It is in these cases, in which the urethra is dilated, thinned, and weakened behind the stricture, that the catastrophe of rupture and urinary extravasation is especially prone to happen when the distended bladder has not been relieved by surgical treatment."

ART. 105.—*On the advantages of Silver Catheters.* By Mr. HENRY THOMPSON, Surgeon to the St. Mary-le-bone Infirmary.

(*Lancet*, June 9, 1855.)

Mr. Thompson believes that there is no instrument which is half so efficient or useful, and none which causes so little pain to the patient, as a well-polished silver catheter. "I am aware," he says, "that in this matter very high authority may be quoted in favour of elastic or flexible instruments. In general terms, doubtless, every man will succeed best with that instrument to which he has been most accustomed, and we may not forget that the flexible instruments were much more in vogue at the period when the authorities referred to acquired their earliest practice, than at the present day, and that thus a practical predilection for them was originally attained, which never could be altogether lost. Nevertheless, what does Sir Benjamin Brodie advise in the treatment of cases of retention from stricture? In the first instance, the use of a gum catheter, and if this fails, a resort to the silver one. But if the latter be the most efficient instrument, surely there is no good reason for not applying it at the outset. If in any class of affections it is desirable to lose no time, to "put our best foot forward"—to make every effort in order to succeed at once, it certainly must be in those which require catheterism, since failure to succeed at the first attempt almost invariably increases the difficulty of a second, on account of the additional irritation which must, to a greater or less extent, be set up. The general objection to the use of a solid instrument consists, I believe, in the fact, that it is possible to do greater mischief with it than with the flexible instrument; that unless properly used, its point may be run through the sides of the urethra, and false passages may be made more readily than with the elastic gum catheter, or wax bougie. A very good argument truly against trusting a man, wholly ignorant of the use of the catheter, with one which is inflexible and solid—a reason doubtless sufficient for recommending the use of the gum instrument to a patient whom you may desire to employ the catheter for himself; but certainly



no reason at all for depriving the surgeon, whose proper function it is to understand its management, of the silver catheter. Granted that it is possible to do more mischief with a solid than with a flexible instrument, I have no hesitation in saying that the former is in an equal ratio as much more capable of effecting good, if rightly used—is as much superior to the latter in its capability of overcoming a difficulty, as the latter is guiltless of power to do much mischief. The case resolves itself into a solution of these questions:—Does the operator desire to control the point of the instrument he introduces into the urethra, in order to overcome some obstruction there? Does he desire to be cognizant of the exact course it is taking there? Can he derive from its point sensations, appreciable through his hand, which inform him as to the progress of the instrument, or the nature of the tissue it encounters, and which may guide him in directing and modifying the motions he communicates to it? If the answer is affirmative to each one of these inquiries—and I presume no one will venture to say it should be otherwise—why should we employ an instrument which bends and twists, so that shortly after its introduction into the urethra, it is impossible to know the curve which it possesses, the direction of its point, or how to obtain those delicate perceptions of position, of the condition of the urethral walls, of the nature of the obstruction, which, with the solid instrument, are so beautifully appreciable, and become so advantageous to the operator. If from any circumstance I feel myself so hopeless of success, that my chance of overcoming the difficulty by the exercise of design and tact in the management of the catheter is gone, I might then be induced to try a flexible instrument, in the faint hope that its point might penetrate, by happy chance, the opening which skill had failed to hit; as the lost rider, in a gloomy night and in a strange country, throws the rein upon his horse's neck, and trusts to fate."

(C) CONCERNING THE UPPER EXTREMITY.

ART. 106.—*Dislocation of the Humerus backwards into the infra-spinous Fossa.* By MAURICE COLLIS, Surgeon to the Meath Hospital.

(*Dublin Quarterly Medical Journal*, Aug., 1855.)

A case of this extremely rare dislocation occurred at the Meath Hospital, in October, 1851.

CASE.—The subject of this accident was an old woman, very thin, with weak flabby muscles. The accident occurred thus: as she was walking along the pathway, with a bundle under her arm, she slipped off and fell forwards on her shoulder; she immediately came up to the hospital, feeling that her shoulder was hurt.

Upon stripping the shoulder the very remarkable symptoms of dislocation backwards were at once readily perceived. In place of the natural rounded prominence in front, there was a deep depression or pit, into which the finger could be pressed; there was flattening of the shoulder on the outer side, below the acromion, and a large rounded prominence was felt at the back of the scapula, below the spine. This prominence was subcutaneous,

and was easily ascertained to be the head of the bone, upon rotation of the arm. The elbow projected forwards and a little out from the side; the axis of the limb ran from the prominence above mentioned downwards and forwards; the length of the limb, from the tip of the acromion to the point of the elbow, was not altered. The patient was either unable or unwilling to attempt motion of any kind, and when desired to do so she moved the scapula on the trunk. We were, however, able to rotate the arm freely, to approximate it to the side, and to bring it forward. We could not raise it or bring it in a backward direction without rotation of the scapula. In our manipulations we experienced no difficulty from the occurrence of tumefaction or effusion, owing to the recent nature of the accident, nor did the patient complain of much pain. The dislocation was readily reduced. Mr. George Porter made extension by raising the arm to a right angle with the body, and drawing it outwards and slightly forwards, at the same time rotating it. I fixed the scapula with the palms of my hands, and made pressure on the displaced head of the bone; with very slight effort the bone returned to its natural place, and the symptoms of dislocation disappeared. The patient recovered the use of her arm at once, and did not return to the hospital.

“All surgical authorities are agreed upon the extreme rarity of this form of dislocation,—not more than eight or ten being on record. Boyer attributes this rarity to the fact that muscular action has no part in bringing about this dislocation. According to him the accident occurs by a fall on the side with the arm extended and advanced; and it will require a very considerable force to be applied to the elbow before the bone can be thrust outwards or backwards; it is manifest, however, that even when the accident occurs in the manner described by Boyer, that the action of the muscles, which attach the scapula to the trunk, largely assist in producing the dislocation. By these muscles the scapula is fixed, while at the same moment the humerus is converted into a powerful lever of the first order. Its centre rests on the side of the chest, the violence is applied at the elbow, and it is only when this violence is sufficient to rupture the capsule, and overcome the action of the muscles about the capsule, that dislocation can occur. The muscles which fix the scapula assist in causing the accident, for if the glenoid cavity were not fixed by them, the violence applied to the elbow would cause it to follow the head of the bone in its movements, and render dislocation impossible. The possibility of dislocation by a direct blow on the front of the shoulder does not appear to have struck Boyer, nor do I well know how to account for its producing dislocation in the present instance, unless by supposing that the glenoid cavity was altered by age and rheumatic disease. It is well known, these causes are sufficient to flatten the cavity, and give it a greater breadth in the backward direction. In the London Medical Gazette for 1833 a somewhat parallel case will be found, in which an old woman, falling on the front of the joint, dislocated it backwards. From the feel of the joint, when reduced, both Mr. Porter and I were of opinion that the dislocation would be easily reproduced; the patient, however, never returned to the hospital, and we are ignorant of her subsequent history. I have thought it right to put the case on record, as the accident is rare; but I regret that I am not able to throw more light upon what may be called the mechanism of its occurrence.”

## (D) CONCERNING THE LOWER EXTREMITY.

ART. 107.—*On exercise in Hip-disease.* By Dr. E. S. COOPER.*(Dublin Medical Press, July 11, 1855 ; and Trans. of the Illinois Med. Soc., 1855.)*

The following case is intended to show the beneficial results of exercise in cases of hip-disease, if the joint itself is kept motionless by some appropriate apparatus.

CASE.—Master John Fear, æt. 9, was attacked in the spring of 1852, with pain in the knee, which continued for some weeks, when it was ascertained that the seat of disease was in the hip, and his physician had him confined to bed, and kept in this position from the 1st of May to the 27th of June, when he was admitted into my institution. I found him in the following condition: much emaciation; pain in the hip and knee; the foot of the diseased side projected two inches beyond the other, when they were placed side by side. Pressure on the heel produced an immediate reference to pain in the hip-joint. Having already witnessed the benefits of early walking in white-swelling, the great relief from pain which exercise gives in these cases after the inflammatory symptoms have been principally subdued, and its invigorating influence upon the general health, I concluded that, inasmuch as keeping the joint quiet was the only object in confining patients to bed generally who have this disease, an apparatus might be devised which would secure the quietude of the diseased parts, and at the same time permit the balance of the body to be exercised, all which I was able to effect by a proper machine. From the period of its application, the patient was more comfortable, particularly during the night; in fact the change was very striking, so much so that from the most painful, sleepless nights, he passed to complete quietude during that period, interrupted by occasional paroxysms of pain, which were readily relieved by an opiate. With this apparatus, I could abduct the head of the thigh bone to the extent desired, and by thus securing the ulcerating articular surfaces from pressure upon each other, and keeping the thigh bone from motion, while with the leg held in a state of flexion, the patient could exercise on crutches without the least detriment to the diseased limb. The general health improved very rapidly, and the appetite became good, while the little fellow began to pass his time quite happily. About this period, however, his father removed him from town. I learned subsequently that he continued to improve after leaving me, and though I am unable to state whether he ever recovered entirely, I think no case occurring in my practice ever gave me more satisfaction at the time. Subsequently, I have treated several other cases with similar results, and though some circumstance in each case has prevented me from witnessing the course throughout, there were none in which the patient did not begin to improve upon the application of the abduction splint. With one on the third day, he was able to press the foot of the diseased side upon the ground in walking, a movement which he had been unable to make for months previously. This case was that of Jotham Lyons, of Fulton county, Illinois, aged fourteen years who had been attacked about seven months when he was admitted into my institution. I shall not give a history of his case in detail; suffice it to say, that, though the symptoms progressed slowly, the disease had gradually advanced from the commencement, until after the application of the splints, since which he has been steadily improving up to the present period.



ART. 108.—*Successful case of amputation at the Hip-joint.*  
By Mr. TATUM, Surgeon to St. George's Hospital.

(*Medical Times and Gazette*, Aug. 18, 1855.)

The case necessitating this operation was malignant disease of the femur, the particulars of which will be found below :

CASE.—A. N—, a youth, æt. 17, with light hair, blue eyes, and an extremely pale complexion, was admitted into St. George's Hospital, under my care, on the 26th of June, 1855, with a large tumour just above the left knee. The diseased mass occupied the whole circumference of the thigh ; was most prominent on the inner side, and extended from the knee one-third up the limb, but the joint itself was quite healthy. The skin over the tumour was tense and shining, with numerous large veins ramifying below the surface. Fluctuation, as of fluid contained in a large cyst, existed over a great part of the tumour.

The history of the case was a brief one. It appeared that about last Christmas, he received a blow over the inner condyle, to which no importance was attached until the beginning of April, when feeling some uneasiness in the part, the patient examined it, and found a slight enlargement over the inner condyle, smaller than a pigeon's egg, and immovable. The tumour did not increase much at first, nor was it painful, but it soon began to grow rapidly, and was then accompanied by great pain, especially at night. The pain, from his description, seemed to have arisen more from the tension of the parts than from any intrinsic condition of the tumour.

On his admission, the patient appeared to be greatly exhausted, and very much wasted, from the great demands of the morbid growth on the frame for nutrition, and partly from the pain and want of sleep. The skin throughout was perfectly blanched ; the pulse 120, not quite devoid of power ; the tongue clean ; and the appetite pretty good.

It being decided to remove the limb, the operation was performed on the 4th of July. The patient, having been placed on the table in a half-reclined posture, was supported by a person seated behind him, and the nates were brought well over the edge of the table. When the patient was fully under the influence of chloroform, the inguinal artery was taken charge of by Mr. Prescott Hewett, who simply compressed this vessel against the pubes, by means of his thumbs. The thigh being slightly bent, and abducted, a long, straight, amputating knife was introduced at the union of the upper and middle third of a line, drawn from the anterior superior spine of the ilium to the great trochanter ; the knife was then carried obliquely inward and downward, immediately over the capsule of the joint, and brought out about two inches below the tuberosity of the ischium. Cutting my way out, I made a large anterior flap, which was at once firmly grasped, so as to prevent all possibility of hemorrhage, and drawn upwards. The anterior part of the capsular ligament being laid bare, was easily divided ; the head of the bone was then partially dislocated by rotating the limb outwards, and extending it backwards ; the ligamentum teres was at once cut through, and the dislocation of the thigh-bone completed. The knife was then carried through the joint, and over the trochanter, and the posterior flap was made. In doing this, I took care and managed so as to have this flap somewhat small and thin, well knowing, by experience, that a large posterior flap tends, by its weight, to drag the

cut surfaces apart in the progress of healing. As I completed the posterior flap, dry sponges were stuffed into the wound by assistants, and thus all hemorrhage from the numerous divided vessels was at once stopped. Such being the case, the femoral artery, and the branches in the anterior flap were first tied, and afterwards those in the posterior flap. A great number of vessels in all were secured, every small branch being taken up. to avert the possibility of secondary hemorrhage. The femoral vein also was last tied, as it continued to bleed freely. The edges of the flaps were then brought together, and retained in apposition by four or five sutures; and some strips of adhesive plaster were then applied, and the whole covered by some water dressing. Some faintness occurred during the operation, but this subsided shortly after the patient had been laid quite flat, and some brandy and water had been given. He was then removed into a spare room, and complete quiet was enjoined.

Immediately after the operation a longitudinal section was made through the soft structures on the front of the thigh, the incision being carried through the bone, and the soft structures behind it, when extensive malignant disease of the bone was observed. This disease was a combination of the medullary, hæmatoid, and osteoid cancers.

On the front of the thigh was a large cyst, containing about three pints of dark, grumous blood, mixed with a thin, dark, and sanious fluid. The cyst was situated just external to the periosteum, and had displaced the extensor muscles from the front of the thigh. A second cyst, containing about half a pint of similar fluid, was found at the back part and outer side of the shaft of the bone.

The upper third of the thigh-bone, and its periosteum, were healthy, but the lower two thirds were extensively diseased. The superficial lamellæ of the bone were separated, and a soft medullary deposit, mixed with granules, and spiculæ of bone, was found occupying the interspaces between them. Towards the lower part of the bone, the separation had taken place to so great an extent, from the amount of deposit, that the superficial layers of bone were quite destroyed, and a large, solid, partly bony, and partly fibrous, mass projected from the back part of the shaft of the bone. The attached portion of the new bone was moderately firm, and arranged in the form of laminæ and plates, in the interspaces between which the fibrous element of the growth was deposited. The surface of this growth was covered with a mass of soft, brain-like, medullary deposit. The remaining part of the compact wall of the bone was much thickened, and towards the lower part of the bone to so great an extent, that the medullary canal was obliterated by a dense osseous tissue, of ivory-like hardness and density.

A careful microscopic examination was made of the several parts of this disease, the result of which clearly showed it to be formed of medullary cancer deposited between the lamellæ of the bone, and between the bone and the periosteum; the formation of hæmatoid cysts in various parts, and the development and growth of osteoid cancer between the lamellæ and on the surface of the femur, corresponding to the lower part of the bone.

After the operation he suffered but little pain; slept some hours on that night; and took beef-tea on the following day. In the evening of the 6th, as there was pain in the stump, and fear of a restless night, a slight morphia draught was given, and was repeated during the night; this gave him several hours' good sleep. The tongue was clean; but the pulse was 120, and languid. A roast slice of meat was ordered, and some porter. On the 8th, the bowels not having acted since the operation, one drachm of castor-oil was given, which acted several times during the day. On the 9th, meat was

given twice a-day, and, in addition to the porter, six ounces of port wine daily. The wound was dressed, for the first time, on the 5th day after the operation, and the sutures removed, and it was found that a large portion of the wound had healed by first intention; there was, however, a good deal of suppuration, which escaped through the openings where the ligatures had been brought out. This suppuration diminished as the ligatures came away; and from the 16th day, when the ligatures from the femoral vessels had separated, the discharge greatly subsided. On the 1st of August he left the hospital well, just four weeks after the operation. Twice during the healing of the wound, the granulations assumed a pale, flabby appearance, which a little grey powder at night, and a rhubarb draught in the morning, completely corrected. The morphia draught, though reduced in strength, was continued during the time that he was in the hospital; he slept uniformly well under its influence; his tongue continued throughout clean, and he took his food with good appetite.

ART. 109—*Fracture of the neck of the Femur, cured without any kind of apparatus.* By M. RIBES.

(*Gaz. Hebdom. de Méd. et Chir.*, May 11, 1855.)

The case is given very briefly. A woman, æt. 63, fell and fractured her left cervix femoris. When M. Ribes first saw her she had lain in bed without any kind of surgical assistance for two whole months; and, at the time, the limb was turned outwards, and somewhat shortened and flattened over the joint. Still she had recovered so as to be able to get out of bed occasionally, and move about the room. The advice which M. Ribes gave was simply to move about as little as she could, to use crutches, and when in bed to put a high pillow under her knee, so as to keep the leg half flexed upon the thigh. This she did, and shortly (dates are not given) she was able to move about briskly with the aid of a simple stick.

In commenting upon this case, M. Ribes says he does not think he should have acted differently if he had been called in at the first, because he is by no means satisfied that a better result would have followed the use of any kind of surgical apparatus. He thinks the inconveniences are great, and the benefits doubtful, in almost all the cases in which the parts are kept upon the stretch by apparatus; and in this opinion he is borne out by Sabatier, Dupuytren, and Astley Cooper.

ART. 110.—*A new apparatus for fracture of the Thigh.*  
By Mr. WINCHESTER, Surgeon to the Westbourne Dispensary.

(*Medical Times and Gazette*, Sept. 1, 1855.)

This is Liston's instrument, so modified as to secure the carrying out of that "principle of adjustment" which Mr. Chichester considers to be of such essential moment in the treatment of fracture. We give the description of the apparatus, and the mode of applying it, as well as a portion of a letter from Mr. de Morgan, Surgeon to the Middlesex Hospital, in which he speaks of its practical advantages.



The instrument consists of five pieces : (*a*) thigh, (*b*) knee, (*c*) leg, (*d*) footpiece, (*e*) swing, (*f*) rack for regulating the angle of the foot-



board, (*g*) is the support for stump-rest, (*hhh*) points or indices, (*fff*) central line, (*k*) screws for regulating length and adjustment.

Its application is as follows:—Apply it to the sound limb, and having fixed the knee and footpieces at the desired angle, regulate the length and adjust to the natural curve by giving the necessary lateral movement, so that the limb may rest in its entire length exactly in the centre of the splint. Having fixed it in this position by means of the screws underneath, remove it from the limb, and observing the indices, mark their deviation from the central line, and, by slightly loosening the screws, turn them to similar points on the opposite side, so that the natural curve of the injured limb may be thereby obtained; or, if preferred, as more in accordance with the ordinary method of procedure, the length may be regulated by admeasurement, and such lateral motion given as is necessary to preserve perfect co-aptation.

Mr. de Morgan writes to Mr. Winchester as follows:

“I have used your double inclined adjustable splint in five cases of fracture in the Middlesex Hospital,—four of fracture of the tibiæ, and one of fracture of the lower end of the femur, extending into the joint. The fractures of the tibiæ were all oblique, and in three there was a strong tendency to displacement, until they were placed on your splint, after which they gave no further trouble. The last case in which it was used by me in the hospital will serve as an illustration of the advantage of attending to the principle carried out by you in your mode of adjusting your apparatus. It was that of a soldier in the Light Dragoons, who had fractured the tibia obliquely at its lower third. The limb had been placed in good position on a Macintyre’s splint; but when I saw the patient, two hours afterwards, the bone was displaced, the lower portion being drawn upwards and inwards. It seemed a good case for testing your splint, and on adjusting it to the sound limb, we found that a considerable inclination of the lower joint of the splint inwards was necessary, in order to fit it properly to the natural curve of the leg: an equal inclination was then given in the opposite direction, so as to fit it for adjustment to the fractured limb, and on now placing the leg in it, and putting the fractured ends of the bone in apposition, they remained in place; nor was it found necessary to interfere further until union had taken place—at the end of four weeks—when the limb was found to be quite strong, and without the slightest irregularity in form or length; yet during the time he was in the splint, the man changed his posture easily and frequently. This he was enabled to do in consequence of your contrivance of a moveable rest at the lower end of the splint, which seems to me to answer even better than the suspending apparatus usually employed. This is the history of the other cases in which I have used the splint, and in all the patients have found great comfort from its application. In one case, I was obliged to remove it from a patient who had been using it for some time, and when there was no longer any risk of displacement of the bones, in order to apply it in a recent case. The patient from whom it had been removed, and whose limb was placed on an ordinary Macintyre’s splint, said that he had found very far greater ease in using your apparatus. It cannot, I think, admit of doubt that the adjustment of the splint to the natural curves of the bone is greatly calculated to ensure a good result. This is easily and effectually done with your splints, but can be effected only

with difficulty, if at all, with any other with which I am acquainted ; indeed I am not aware that the attempt had been made, or the principle recognised, before your invention of the adjusting long splint. The moveable rest is hardly less useful, as it allows the patient to move and shift his position without risk of displacement of the fractured bones. I have not found anything to object to, nor can I suggest any improvement in your splint."

ART. 111.—*On amputation of the Thigh in civil and military practice.*  
By Dr. RICHARD MCSHERRY.

(*American Journal of Medical Science*, Jan., 1855.)

The great fatality which attends upon amputation of the thigh after gun-shot wounds is evidenced by the fact, that, during the campaign in Mexico, under General Scott, there were no recoveries, so far as Dr. McSherry could learn, after the operation. This mortality is not peculiar to America, as M. Ribes examined four thousand invalid soldiers, among all of whom he did not find one single instance of injury of the femur by shot, nor one who had undergone amputation of the thigh. Malgaigne was equally unfortunate with all his cases during the Polish campaign. In civil life the case is widely different. The statistical tables, published by Dr. Norris, state that during a period of ten years, of sixteen amputations of the thigh, performed in the Pennsylvania Hospital, fourteen recovered, and but two died. The two fatal cases were primary amputations, after fracture of the thigh and compound and comminuted fracture of the leg respectively. Of the fourteen successful cases, seven were fractures (one ununited fracture of thigh), one tumor on the knee, and six caries of knee-joint. These results are favorable, Dr. McSherry thinks, to secondary amputations.

In the Parisian hospitals, there were, in ten years, forty-four amputations of the thigh. Of these, thirty-four died, and ten recovered. All of these, however, were amputations after *traumatic* lesions. A comparison of these results shows that patients who undergo great operations, in full robust health, are more liable to perish than those who are subjected to them for chronic disease. There is abundant proof also, that delay is advantageous. Dupuytren, in 1830, treated thirteen cases of fracture of the thigh *without* amputation ; of these six recovered, and eight died. Malgaigne reports five cases treated by himself *without* amputation, of whom two recovered, and three died. These results, compared with those in the same hospitals after amputation, show that the chances of life were greatest where the operation was not only deferred, but foregone altogether. Dr. McSherry argues from these facts that the rule, which requires immediate amputation after a gun-shot fracture of the femur, is a bad rule, inasmuch as it sacrifices the limb without adding appreciably to the chances of ultimate recovery. When there is complete disorganization of the limb from the shot, the case is, of course, different. The *exception*, however, should not be made the *rule*.



ART. 112.—*On internal derangement of the Knee-joint.*

By Mr. SYME.

(Lancet, May 5, 1855.)

The following remarks upon a rare affection, to which attention has been recently called by others, (Abstract, xxi, p. 185), occur in a clinical lecture delivered at the Royal Infirmary at Edinburgh :

“Many years ago, gentlemen, in reading the observations of Mr. Hey, of Leeds, I was much struck with some cases mentioned by him under the title ‘Internal Derangement of the Knee-joint,’ in which the articulation was affected with some obscure injury, producing more or less lameness, and interfering with the perfect motion of the limb, and remedied by acute flexion, followed by sudden extension of the leg. From that time I had been on the look out for such cases, without ever meeting with one, and though I never doubted Mr. Hey’s accuracy, I could not but wonder that that of which he had seen several examples should never have occurred in my practice. The other day, however, a case of this kind came under my care, and although you did not see it, I think it would be wrong if I omitted to allude to it; but before speaking of it, I will read you the account given by Mr. Hey of one of his cases.

“In 1784, the Honourable Miss Harriet Ingram, as she was playing with a child, and making a considerable exertion, and stretching herself forward, and stooping to take hold of the child while she rested upon one leg, brought on an immediate lameness in the knee-joint of that leg on which she stood. The disorder was considered as a simple sprain, and a plaster was applied round the joint. As the lameness did not diminish in the course of five or six days, I was desired to visit her. Upon comparing the knees, I could perceive no difference, except that when the knees were placed in a state of complete extension, the ligament of the patella of the injured joint seemed to be rather more relaxed than in that joint which had received no injury. When I moved the affected knee by a gentle flexion and extension, my patient complained of no pain, yet she could not perfectly extend the leg in walking, nor bend it in raising the foot from the floor, but moved as if the joint had been stiff, limping very much, and walking with pain. I thought it probable that the sudden exertion might in some degree have altered the situation of the cross ligaments, or otherwise have displaced the condyles of the os femoris with respect to the semilunar cartilages, so that the condyles might meet with some resistance when the flexor or extensor muscles were put into action, and thereby the free motion of the joint might be hindered when the incumbent weight of the body pressed the thigh-bone closely against the tibia, though this derangement was not so great as to prevent the joint, when relaxed, from being moved with ease. To remedy this derangement, I placed my patient upon an elevated seat, which had nothing underneath it which could prevent the leg from being pushed backward towards the posterior part of the thigh. I then extended the joint by the assistance of one hand placed just above the knee,

while with the other hand I grasped the leg. During the continuance of the extension, I suddenly moved the leg backwards, that it might make as acute an angle with the thigh as possible. This operation I repeated once, and then desired the young lady to try how she could walk. Whatever may be thought of my theory, my practice proved successful, for she was immediately able to walk without lameness, and on the third day after this reduction she danced at a private ball, without inconvenience, or receiving any injury from the exercise.\*

"The case to which I wish to direct your attention was that of a young man, about thirty years of age, who came from the country to consult me on account of stiffness and pain in the right knee, of four or five days' standing. He had been for some years liable to attacks of this kind, but they had generally passed off quickly. His case differed from that of the young lady mentioned by Mr. Hey, inasmuch as it was impossible to extend the limb completely, even by external force, but, as in Mr. Hey's case, there was nothing to be felt wrong about the joint, except a loose cartilage in the pouch above the patella, freely movable, and evidently having nothing to do with maintaining the derangement of the knee. As movement of the joint to any considerable extent caused acute pain, I placed the patient under the influence of chloroform, and following Mr. Hey's principle, bent the limb to the full extent, and then attempted to extend it, but at first did not succeed in doing so completely; but after repeating the process several times, and shaking the limb in every direction, while the muscles were completely relaxed, I felt something give way in the joint, and then immediately found myself able to extend the limb completely, and the patient was from that time free from lameness. Mr. Hey speaks of some displacement of the crucial ligaments or semilunar cartilages as the probable cause of the lameness in these cases; the latter appears to me the more likely explanation; but, as Mr. Hey says, whatever may be thought of the theory, the practice proved successful."

ART. 113.—*Excision of the Knee-joint.* By (1) Mr. PEMBERTON, Surgeon to the General Hospital at Birmingham; and (2) Mr. HENRY SMITH, Surgeon to the Westminster General Dispensary.

1. (*Assoc. Med. Journal*, May 18, 1855.)

2. (*Medical Times and Gazette*, May 26, 1855.)

Both these cases were successful, and the recovery retarded by no accident. Mr. Pemberton's case also presents an additional feature of interest, in that the mode of operating was that adopted by Milder of Gröningen—a mode not before carried out in this country. This mode is to cut through the bones from before backwards by the careful application of the saw, without disturbing the soft parts by the previous introduction of a spatula.

1. *Mr. Pemberton's Case.*—Edwin Fowl, æt. 12, pale, and suffering from strumous disease of the left knee for fourteen months. The operation was

\* Vide Hey's *Observations in Surgery*, 1810, p. 886.

performed in the General Hospital, at Birmingham, on the 8th February, 1854.

“An incision was carried from a little above the outer to a little above the inner condyle, across the front of the joint, below the patella, dividing its ligament of insertion down to the spine of the tibia. The flap thus formed was turned back; and the cavity of the joint was fairly exposed. The disorganized soft parts having been cleared away from over the femur, it was sawn through above the condyles, without the aid of a spatula, or the introduction of a knife. The same process was next applied to the head of the tibia; and the articular extremities were then removed in their connected state, by a cautious dissection of the soft parts beneath, commencing from above downwards. The hemorrhage was inconsiderable; no ligature was required. The amount of bone removed measured rather more than three inches and a half. About two inches and a half belonged to the femur, and about an inch to the tibia. The patella was left, its under surface being scraped. The head of the fibula was not interfered with. The operation being finished, the leg was readily brought into a line with the thigh. The flap containing the patella was simply laid, in as accurate a state of apposition as possible, over the parts beneath, without the aid of sutures, and was covered with water dressing. The entire limb was then adjusted on a straight splint, reaching from the buttock to the ankle, furnished with a foot support, and with side pieces to the thigh and leg; the knee being left perfectly free on the sides and above, for the application and renewal of dressings.

“*Examination of the diseased parts.*—The synovial membrane was everywhere affected by disease. It presented a pulpy, thickened condition, and was of a brownish tint, and covered in places by bloody discolorations. The cartilage covering the tibial surface of the outer condyle was destroyed, as was also the corresponding surface of the semilunar cartilage of the tibia. The entire thickness of cartilage on the inner condyle and tibia was not altogether destroyed, but was in process of ulceration. The extremity of the femur above the condyles was blackened, and denuded of periosteum. The bone was soft and carious. Fresh osseous material had been thrown out behind the condyle and on the head of the tibia. The medullary canal of the bone did not present an unhealthy appearance.

“No shock followed the operation; and but a single restless night marked the presence of any constitutional disturbance. The warmth of the limb, below the seat of operation, never varied. The patient had scarcely any pain, and could in the course of a fortnight exercise complete command over the muscles of the foot and leg.

“Three months afterwards, the following note was made:

“The healing of the wound had been retarded by the thickened character of the integuments. The parts in the situation of the joint were becoming firmer and more consolidated. The boy possessed perfect control over the movements of the foot, and turned it, with the entire limb, either inwards or outwards. The splint was discontinued, and the wound was firmly strapped and rolled.

“Four months after the operation, the wound remained open slightly at the sides. A gutta-percha splint was applied behind the joint; and the boy was directed to be out of doors on crutches. An accurate measurement was made of the two limbs, when it was found that the difference between them exactly corresponded to the amount of bone removed—namely, three inches and a half.

“The patient remained in the hospital until October; more for the purpose of observation than from any necessity. When discharged, eight months



subsequently to the operation, the wound had entirely healed. One of the sinuses, existing previously to the operation, over the patella, still discharged. He could walk with the aid of a stick and a high-heeled shoe; the knee being supported by a leather case.

"I saw the boy so recently as the 23d of last month (April). The limb was quite straight. The wound was sound. The union appeared to be ligamentous. He could walk about anywhere with his leather case and stick."

2. *Mr. Smith's Case.*—John H——, æt. 7, slim, pale, and suffering from strumous disease of the knee.

"On Wednesday, October the 18th, the patient was placed under the influence of chloroform by Dr. Snow, and the operation was performed by me in the following way:—An incision was carried from the outer condyle of the femur along the side of the joint, in front of the patella, and from thence round to the inner condyle; this incision included the ligament of the patella, which structure, with the patella itself, was dissected up to the large semilunar flaps thus formed. The soft tissues were then very carefully separated from the circumference of the lower part of the femur, the lateral and crucial ligaments were cut through, and the saw being applied the condyles were removed. Great care was taken to limit the movements of the saw, as the posterior border of the bone was being reached. The tissues around the head of the tibia were next separated, and about a quarter of an inch of this bone was removed, when an abscess of considerable size was seen in its interior, the cavity extending below the seat of section; it therefore became necessary to remove about half an inch more bone, by which measure the cut surfaces were brought into apposition, and the limb drawn into a straight position. As the inner surface of the patella was diseased, this bone was also removed.

"There was not any necessity to tie a vessel; the flap was fastened by several sutures; wet lint, and a bandage from below upwards was applied, and the limb was properly secured in a box made especially for the purpose, consisting of a support with two lateral flaps and a footboard, all of which might be lowered at pleasure without the leg being disturbed.

"On examination of the parts removed there was found to be even greater disease than I expected. The cartilages on the condyles of the femur were ulcerated to a great extent, and in the intercondyloid space was a piece of bone about the size of a small nut, which was in a state of necrosis, and nearly detached from the sound bone. The cartilage was off from the head of the tibia, and the interior of this portion of bone was hollowed out by a considerable cavity. The inner surface of the patella was deprived of its cartilaginous covering partially, and the synovial membrane was in a state of pulpy degeneration.

"14th.—The progress of the case has been perfectly satisfactory up to this point, and to-day the patient has left his bed in greatly improved health; the leg is increasing in size, and the union at the joint is becoming firm; there are some superficial sores in the seat of the sutures, and the sinus in front of the head of the tibia keeps discharging.

"30th.—Two days after the last report, I removed the limb from the box altogether, and, having found it firm and straight, applied a gutta-percha splint, and ordered the boy to move about on crutches; this he does every day about the house. An abscess had formed over the front of the joint, and was opened. The boy has been taking quinine; the sores before mentioned are still open. He can plant the toes of the weak limb firmly upon the ground while standing upright.

"January 17th.—This boy has been allowed to go out into the streets, and

has much improved in general health, but the movement of the limb has been prejudicial, and has caused the sore in front of the tibia to spread and put on a sloughy appearance, and an abscess has formed again in the ham. I have, therefore, desired him to keep quiet, and have opened the abscess.

"February 2d.—In consequence of the severe frost, this little boy has been compelled to keep in-doors; but he is going on improving in health, and the sores have healed considerably. The limb is firm at the cicatrix.


"26th.—The counter-openings which were made in the ham are, I find, connected with the opening in front of the tibia, and there is a slight discharge from each of the three places. A probe passed through the one, readily passes out of the other orifice. I therefore deemed it most prudent to lay the sinuses open, and accordingly did so to their full extent. On this being done, a considerable cavity, like that of an old abscess, was exposed; but it was superficial, and did not communicate with any diseased bone. The patient was ordered to lie up until this wound, which was of considerable extent, should begin to granulate. The limb is very firm, and the circumference is very much increased in size; he can move it about in all directions; and he has such mobility of the hip, that he can throw his straightened leg behind his neck while seated in a chair. He has been living well, and occasionally taking cod-liver oil.

"April 15th.—For the last month, this patient has been walking about the streets every day. After having been furnished with a boot having a sole two inches higher than the other, and by means of a crutch and stick, his powers of progression are very free. He plants the limb, which is very firm, well down upon the ground, and with the assistance of a stick alone, he can walk with the utmost facility; but when he goes out for any distance, I have desired that he should use his crutch as well, as a mere precautionary measure, especially as the boy, being proud of his leg, is very fond of showing his agility, by the performance of the most eccentric movements, which are more calculated to amuse others than to enhance the utility of his limb. The large sore, which had resulted from the opening up of the sinuses in the ham, is healing, and the cicatrix of the original wound is firm and healthy. On very careful admeasurement of the two limbs, I find that the limb operated on is two inches and one quarter shorter than the other. It is straight, and not bowed out. On using some force above and below the site of operation, a little amount of movement, before backwards, can be produced, but in the lateral direction this is hardly detectable. In all probability the junction between the bones is partly fibrous, partly osseous. Whatever be its nature, the limb is a remarkably useful one to the boy.

ART. 114.—*On the treatment of Onychia.* By Mr. HUMPHREY.

(*Philadelphia Medical Examiner*, Aug., 1855.)

The plan here proposed is very simple. It is nothing more than a piece of silver, rolled out sufficiently thin to admit of being bent to the required shape, yet sufficiently firm to bear moderate pressure. This should be nearly the length of the nail, from a quarter to half-an-inch wide, and bent into somewhat of an S shape, or rather

thus, . The lower end (*b*) is, with the aid of a pair of forceps, to be carried down between the overhanging ulcerated skin and the nail,

and hooked under the rough edge of the latter. The upper end (a) is then carried outwards and secured in that position by a strip of plaster, and a bandage round the toe. By this means, the inverted edge of the nail and the skin are effectually kept from one another, and pressed in opposite directions.

“The nail is a little elevated, and the ‘fungous growth’ very soon shrinks under the pressure of the metal, and assumes a healing aspect. Often, when the silver is well adjusted, the patient is able to walk about with comparative ease immediately afterwards. I do not interfere with it for several days, when a marked improvement is usually found to have taken place. The silver is readjusted with greater ease, and allowed to remain a longer time. Gradually the ulcer heals, and the nail grows up in more natural shape and appearance. It is well, however, to continue the use of the silver for some time; and, after the sore has quite healed, it is well to insert a piece of lint, or a small flat piece of silver, under the edge of the nail, to prevent the tender cicatrix being fretted by it, and to keep down the skin. The patient should be directed to avoid tight shoes, and not to cut the corner of the nail low down. In some bad cases, it has been necessary to keep the patient quiet, or in bed, for a short time; and, in a few, to prepare the way for the silver by the introduction of a piece of lint, secured by a strip of plaster.”



# PART III.

## MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

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### (A) CONCERNING PREGNANCY AND PARTURITION.

ART. 115.—*The true mechanical relations of the Sacrum.*  
By Dr. J. MATTHEWS DUNCAN.

(*Edinburgh Medical Journal*, Aug. and Sept., 1855.)

Dr. Duncan's purpose in this paper is to show that the sacrum cannot properly be compared to a wedge or to the key-stone of an arch; and his arguments, we think, are very conclusive. In Dr. Duncan's opinion, "the sacrum is to be regarded as a strong transverse beam curved on its anterior surface, and having its two ends, being the two articular surfaces, in contact with the corresponding parts of the iliac bones. It is so formed and placed between the two iliac bones, that, under the weight of the body or any vertical pressure, it cannot act as a whole like a wedge. But, as already pointed out, there are certain limited parts of the auricular surfaces of the sacrum so arranged as really to have a wedge-like action. These are so limited, however, as not to be of nearly paramount importance, but act as what are called 'bites' in engineering.

"The whole weight of all the upper parts of the body is transmitted through the vertebral column to the sacrum. How then is the sacrum retained *in situ*? Above and behind the articular surfaces, are attached the posterior sacro-iliac ligaments which unite these parts to the corresponding rugosities on what may be called the posterior iliac tuberosities. From the former to the latter parts, the fibres of this, which is the strongest ligament in the body, run obliquely upwards and outwards. The sacrum has nothing to prevent its being depressed under the weight of the body, but these ligaments, to which the weight is all transmitted, and again through them to the posterior iliac tuberosities. The whole weight of the body then on the sacral beam is suspended by the sacro-iliac ligaments from the posterior iliac tuberosities, which, projecting backwards and inwards, overhang the sacrum to some extent.

"Once transmitted to the posterior iliac tuberosities, the weight is by them carried to the heads of the femurs through the iliac or cotylo-sacral beams which lie between them. The iliac or cotylo-sacral beam then extends from the posterior iliac tuberosities to the acetabula. This beam is pressed against the sacrum, as against a

fulcrum, by forces applied at both its extremities. At the upper extremity, the force is one dragging in a direction corresponding to that of the posterior sacro-iliac ligaments, more or less in a line towards the centre of the pelvic circle. The force at the lower extremity is the reacting force of the weight of the body pushing in a direction upwards and inwards, also towards the centre of the pelvic circle. Both of these forces press the beam upon the fulcrum or auricular surface of sacrum, which, it is of importance to observe, is not in the centre of the beam, but nearer its upper than its lower end. The articular surface is, in mere distance, but little nearer the upper than the lower extremity of the beam, but mechanically viewed, is much nearer; for while the force applied to the lower end of the beam is applied to its extremity only, that applied to the upper is applied over its whole surface above and behind the auricular portion, and therefore the length of this arm of the beam is counted, in the mechanical problem before us, only to the centre of the parts to which the force is applied."

ART. 116.—*On the condition of the neck of the Uterus during pregnancy.* By M. CAZEAU.

(*L'Union Médicale*, April 21, 1855; and *Edin. Monthly Jour.*, June, 1855.)

Such is the title of a memoir read by M. Cazeau before the Société de Chirurgie, and upon which M. Laborie has drawn up a report, with an interest and labour seldom bestowed upon this task, in the learned societies of the continent.

The neck of the uterus, in the pregnant female, has often been subjected to examination, as far as that can be made by touch alone; but it is only of recent date that another method of investigation, in this respect, has been afforded, in the employment of the speculum vaginæ, and it is the results obtained from researches by means of this instrument, that M. Cazeau describes. According to these observations, he states that, in primiparæ as well as in multiparæ, the vaginal portion of the neck of the uterus is of a dark-red colour (*lié-de-vin*), and in primiparæ, its whole surface presents a smooth appearance. The os uteri, of which the lips are very soft, is in general more or less rounded, and of a larger size than in the unimpregnated condition. The free portion of the neck in a very few cases exhibits ulcerations, and more frequently granulations of a cherry-red colour, and bleeding easily. Among the multiparæ, the neck is larger; the opening is divided into different portions; it is large, and the interior of it admits of being examined. The walls of this part of the organ are irregular, and present a number of fungous elevations which bleed readily, in the hollows between which are occasionally observed ulcerations of a linear shape, and more or less deep. M. Cazeau regards these ulcerations as of little importance, and disapproves of any treatment for their removal.

MM. Boys de Loury, Costillies, and Coffin, consider the ulcerations of the first period of pregnancy, as having a marked influence in the production of abortions.

Admitting so far this opinion, M. Cazeau rejects it in its application to the ulcerations of the last months of utero-gestation; and,

according to this view, he proposes that no treatment should be adopted in the latter cases. Thus, with the exception of specific ulcers, M. Cazeau considers that the others should not be interfered with, and, unless an excessive tendency to spread were manifested, no local means of treatment should be employed.

ART. 117.—*Influence of starvation on the part of the mother upon the size of the child.* By M. P. DUBOIS.

(*Rév. Méd. Chir. de Paris*, June, 1855.)

A case recently occurring in the practice of M. Dubois supports the idea that it may be possible to put some check upon the growth of the fœtus in cases of pelvic contraction, by stinting the quantity of food supplied to the mother.

The patient in question became pregnant about three months after marriage, and from the first she suffered from excruciating pains in the stomach. These pains began immediately after taking food, and never ceased until the stomach became empty. Nothing but abstinence afforded any relief, and the result was that she took scarcely any food at all during the whole period of pregnancy.

Delivery took place about a fortnight before the full period, and the child was extremely small (hardly 1500 grammes). The placenta, also, was very small, but in other respects it was quite natural.

In short, the whole appearance of the child and placenta suggested the idea that the unusual smallness was nothing more than the natural consequence of the peculiar starvation of the mother; and this opinion M. Dubois supports by referring to the case of a woman who had given birth to a large child with extreme difficulty, and who, being again pregnant, voluntarily placed herself upon low diet in the hope of having a smaller child by so doing. We are left to conclude that the woman was not disappointed in her expectations.

The subject is well deserving of attention.

ART. 118.—*The effect of the death of the Fœtus upon the duration of pregnancy.* By Dr. JOHNS.

(*Dublin Quarterly Journal of Medicine*, Aug., 1855.)

Dr. Johns relates the case of a fœtus which died about the sixth month, and was retained to the natural term of pregnancy, and then (among other things) he directs attention to the evil consequences which might arise from ignorantly supposing that delivery had taken place when the fœtus died.

CASE.—Mrs. M., æt. 40, mother of four living and two premature still-born children, when pregnant for the seventh time, engaged me to attend her in her then approaching confinement, which she stated would occur on the 11th of April, 1853, as she had menstruated on the 11th of July, 1852.

On the 12th of February this lady sent for me, in consequence of uterine hemorrhage, which had set in on the previous evening, and was then continuing, but unaccompanied by pain.



I made a vaginal examination, and satisfied myself that she was not in labour. I also examined the abdomen most carefully, and then mentioned to her my conviction that she was not so long pregnant as she believed herself to be, for that the womb had only attained the size and position in the abdomen usual at six months of pregnancy; to which she replied, that she had quickened on the 10th of November, when four months pregnant, and had continued to feel her child up to the end of the sixth month, but that since that period she was not sensible of its vitality. At this visit the pulsations of the foetal heart were not discoverable, but the placental soufflet was indistinctly audible, abrupt, and weak. The hemorrhage ceased, rest being the only means employed.

On the 11th of the following month (March) I was again summoned to Mrs. M., the hemorrhage having returned on that morning, but without pain, as on the former occasion. She being very hysterical, I ordered an anodyne draught, which, together with rest in the recumbent posture, had the desired effect of calming her, and arresting the discharge. I embraced the opportunity of this visit to test the accuracy of my former prognosis, when I was much surprised at discovering that the uterus had not increased in size since my visit of the preceding month, its fundus then having reached as far as the umbilicus, which was protruded. My patient, however, insisted that she had become much larger, which fact, doubtless was attributable to flatulency, which obtained to a great extent, and from which she suffered much. I then made a very careful stethoscopic examination of the whole abdomen, but I could not discover either the foetal pulsation or the bruit placentaire. Borborygmi were, however, very audible, but not in positions likely to mask the other sounds.

The occasion of my next visit to this lady was when labour had set in, which occurred at nine o'clock, p.m. of the 11th of April, 1853. The uterus then held the same position in the abdomen as on my two former visits. Once more a stethoscopic investigation was instituted, but not with happier results than before. Labour progressed slowly till half-past four o'clock, a.m. of the 12th of April, when, with one violent pain, she brought forth the placenta with the membranes unruptured, containing about a pint of whitish fluid of the consistence and appearance of skimmed milk, in which was floating a dead, dried up, and withered foetus, apparently of about six months, presenting very much the aspect of having been for some time macerated in spirits of wine. The placenta and membranes were healthy, and neither they, the child, nor the liquor amnii exhaled the least unpleasant odour. There was not any hemorrhage or other bad symptom after delivery, and she recovered very quickly. Mrs. M. mentioned to me, in conversation, that she had enjoyed much better health than usual whilst carrying this child, and that she had gained flesh.

"What effect, then," adds Dr. Johns, "has the death of the child upon the duration of pregnancy? This, at a superficial view, may appear of little moment; but, on deeper reflection, it shall become self evident that a false prognosis in such a case as the one before us may induce very fearful consequences to the physician, or may embitter the happiness of families, by causing wounds that never may be healed, separations never to be reunited. In illustration, the following case is by no means improbable.

"Captain B., R.N., marries and leaves his youthful bride to join his ship three weeks after his marriage, she then being pregnant, but without his or her own knowledge. Gestation progresses favorably

for six months, at the end of which period the child ceases to live, but is retained in the womb till the full period of natural pregnancy; this viscus not having enlarged after the child's death, the mother never felt her child, nor was she at all conscious of her condition, being necessarily inexperienced in such matters, besides being, as she supposed, unwell each month (which discharge may have depended upon ulceration of the os uteri upon nature's attempts to throw off the dead fœtus, which had become a foreign body, or upon many other causes too numerous here to relate). However, at the end of the ninth month her husband returns, expecting to find a young and spotless wife, to be alike participator of his joys and griefs, when, alas! to his horror and dismay, he finds that she has just given birth to a six months' child, but dead, and in other respects like Mrs. M.'s baby. Is it not natural that he at once accuses his lady of infidelity? and what protestations of hers as to her innocence, be they never so solemn, shall convince him that she still is not guilty? This is the juncture at which the physician may be the balm or the wormwood; therefore upon him rests the responsibility of deciding the question. But if he be ignorant of the possibility of a dead child being carried in utero for such a period, he condemns the lady, and I need hardly say, the consequence is too apparent. If, however, subsequently, the ill-judged, ill-treated, and unfortunate lady's innocence be proved, what shall become of the reputation of that physician who unhesitatingly pronounced her guilty? But if no such happy result should ensue to the lady, what shall and ought to be the feelings of that man, when, in after years, he shall discover his ignorance, and think upon the mischief it had entailed upon society?"

ART. 119.—*Blighted fœtus at the fifth month, retained and expelled with a living fœtus at full term.* By Dr. WILLIAM M. BELT.

(*American Quart. Jour. of Med. Science*, April, 1855.)

This case occurs as an original communication. It is a pity that more is not said about it, but we give all we find.

"I was called," says Dr. Belt, "on the 23d of January, 1852, to see a girl in the employment of Mr. Young, of Independence, Mo., who was supposed to be in danger of abortion. The waters had escaped, according to her statement; and there was slight hemorrhage with strong uterine pains. I kept her in a recumbent position, and gave a mild cathartic. In five or six days she was able to walk about the house, and her usual health was soon re-established. On the 21st of May I delivered her of a healthy child. In tracking the cord to the placenta, I came in contact with a *dead fœtus* of five months."

ART. 120.—*Instrument for dilating the os uteri.* By Dr. BRAUN.

(*Klinik der Geburts*; and *Medico-Chir. Rev.*, Jan., 1855.)

"Dr. Braun has contrived an instrument, which he names *colpeurynter*, for the dilatation of the os uteri. It consists of a vulcanized

india-rubber bladder, from two to four inches in diameter, and four inches in length, with an india-rubber tube enclosed in horn, fitted with a brass stopcock, and a ring through which to pass a silk belt. When used, the india-rubber bladder is introduced empty into the vagina, then gradually distended by injecting cold or warm water. It is retained *in situ* by the belt fastened round one or both thighs or hip. The horn cylinder is curved in the direction of the pelvic axis; it allows only the upper end of the vagina to be stretched by the bladder: and obviates any unnecessary pressure upon the urethra or external parts. The operation is called *colpeuryisis*. The advantages of this proceeding over other methods of dilating the os uteri, whatever the indication for that operation, are highly extolled."

ART. 121.—*A case of Symphyseotomy.*

By M. MASLIEURAT-LAGEMARD.

(*Medico-Chir. Review*, Oct., 1855; and *Bull. Gén. de Ther.*, May, 1855.)

We copy Dr. Barne's abstract of this case.

CASE.—The subject of this case was a woman who had borne two children without anything remarkable occurring. In a third labour (November, 1847), the head was arrested, and symptoms of exhaustion set in; the forceps were tried several times in vain. Turning was then resorted to, the feet were brought down, but the head resisted all endeavours at extraction. The woman had now been three days in labour. Choice lay between Cæsarian section and symphyseotomy. The latter was selected. An incision was made in the median line of the symphysis, the cartilage was then divided by a blunt-pointed bistoury. The separation of the iliac bones was effected by pressing with each hand upon the antero-superior spines of the ilia. A slight crackling was heard in the sacro-iliac articulations, and the two branches of the pubis were found sufficiently parted to admit the finger. As soon as this was done, a very gentle traction on the body of the fœtus sufficed to bring forth the head, and finish the labour. The child was dead. A bandage was applied round the pelvis, to bring the pubic bones together. Three days after, shivering and acute pains in the right leg set in; phlegmasia dolens became developed. The patient recovered, notwithstanding, and renewed her occupations at the end of fifteen days. M. Maslieurat-Lagémard has seen her recently; she has been delivered again since of a living child, well-formed, without difficulty.

It is right to add that M. Maslieurat was unprovided with the instruments requisite for cephalotripsy or for ex-cerebration.

ART. 122.—*Incision of the Vulva for the prevention of rupture of the Perinæum.* By M. CARPENTIER.

[*Révue Méd.-Chir. de Paris*, Feb. 1855.]

Two cases are described by this author, which, if they do not show the necessity of this proceeding, at least point out its efficacy as an operation. It was recommended by Michaëlis in 1810, since which time it has been practised by Weire, Eichelberg, and other accoucheurs,



and does not deserve the neglect in which it has fallen at the present day.

The first case related is that of a primipara where the labour had been protracted, the head of the child having appeared at the vulva, remained there, notwithstanding the strength of the pains, for nearly two hours. M. Carpentier then made an incision, not in the raphé, but laterally in the vulva, and this operation was scarcely completed, when a vigorous pain terminated the labour.

The other case was one requiring the employment of the forceps, which were applied without difficulty; but upon the head appearing at the vulva, the operator saw that extensive laceration of the perineum was threatened. He therefore confided the forceps to an assistant, who maintained them in position, while an incision in the vulva was made during a pain. The child was then extracted without any resistance.

It is well known that laceration of the fourchette is very common in first labours, and that in spite of all the precautions taken in supporting the perineum, it is ruptured in some cases as far as the rectum. Would then, asks M. Carpentier, incision of the vulva prevent such accidents, by substituting a surgical operation for a rent made by nature fortuitously and with violence? Insufficient statistics render any conclusion on this question difficult as yet; experience is required to show its true value as an operation.

ART. 123.—*On the recent epidemic of Puerperal Fever in the Dublin Lying-in Hospital.* By Dr. M'CLINTOCK.

(*Dublin Quarterly Journal of Medical Science*, May, 1855.)

A most serious epidemic of this fever, presenting many peculiarities, raged in this hospital, from the beginning of last December to the end of the following February. Unlike the one of 1845, its outbreak cannot be said to have been either sudden or unexpected, inasmuch as twelve or fourteen cases of puerperal peritonitis and phlebitis, together with a few isolated examples of typhus and scarlatina, had occurred in the house during the preceding nine months.

From the beginning of December to the 14th February, 182 women were confined in the hospital. This is not half the average number of deliveries in the same period, and was owing to a stop having been put, in the latter part of December and during all January, to the admission of patients, except such as were so near delivery that it would have been attended with imminent risk to send them away.

Now of these 182 women, 38, that is 1 in every 5, were unequivocally affected with the symptoms of the disease; and out of these 38 so affected 17 recovered, and 21 died, making the proportion of fatalities nearly 1 in 8 of all admitted; a frightful rate of mortality, and more than tenfold the average of this hospital.

In three of the above cases the puerperal disease was complicated with scarlatina. Two of these died, and the third made an excellent recovery, though the metritic attack was a marked one, and the scar-

latina very severe, showing itself so early as the second day after delivery, and presenting in its course a truly formidable array of symptoms. On two occasions this woman seemed to owe her preservation solely to the liberal exhibition of wine and brandy, and this too at the very time when we had every reason to fear the existence of uterine inflammation.

It would be wearisome and tedious to give the individual history of all these cases, and yet I am quite at a loss how to classify or arrange them, not knowing what to take as the basis of any such classification, as they presented considerable variety in their symptoms, course, and morbid appearances. For example, in many cases, including some of the most malignant, there was no initiatory rigor whatsoever. Again, intense abdominal pain was a prominent feature of some cases from the onset to the termination; whilst in others, equally fatal, there was *no* complaint of the belly. Vomiting, likewise, was an early and constant attendant upon the disease in not a few instances, whilst in some it did not appear at all, or only at the close. And so on with the morbid appearances; some cases presenting intense peritonitis, others phlebitis, and a few putrescence of the uterus, and these either separately or conjointly. There were two features, however, common to them all, namely, a very rapid circulation, the pulse ranging from 120 to 140, and a marked adynamic type; so marked, indeed, that in two cases only did I feel justified in making trial of phlebotomy, and these, as you may suppose, were selected cases. Yet, in each of them, the supervention of syncope rendered it necessary to discontinue the bleeding before ten ounces of blood had been abstracted, one losing about seven, and the other nine fluid ounces; and what is still more worthy of attention, is the fact, that in neither of these instances did the blood exhibit, after some hours' standing, any of the characters indicative of inflammation. Both these patients died.

In nothing did the various cases differ so much as the manner in which the disease made its invasion. In the majority a rigor announced its first onset, this being speedily followed by pain or uneasiness in the uterus; except in three or four instances, the pain was not by any means intolerable or severe at the commencement, or even for some hours afterwards. Tenderness of the uterus to pressure, however, with perceptible augmentation of its bulk, was almost invariably found to be present from an early period of each case.

The first approaches of the disease, when not ushered in by rigor, were sometimes remarkably slow and insidious,—the only deviations from normal convalescence being a trivial acceleration of the pulse and a slightly furred state of the tongue, with, perhaps, diminished secretion of milk. On two or three occasions the attack began apparently with after pains, or at least with pains of an intermitting character, commencing almost immediately after delivery, and so equivocal in their nature, that it was impossible to say when they ceased to be purely spasmodic and became inflammatory. Mr. Hey, of Leeds, in his 'Treatise on Puerperal Fever,' makes the remark, "that during the epidemic season lying-in women were unusually

subject to after pains, and those of a more violent kind than ordinary." My recent experience is quite in accordance with this observation.

The patient's own representation of her state we found could not always be relied on, owing to her unconsciousness of the presence or the progress of the malady. Frequently her statements on this head, though made with confidence and complacency, were yet so utterly at variance with the symptoms and actual condition of the patient, that the most inexperienced observer could scarcely have been deceived by them for one moment. Whether this apparent ignorance of her real state arose from an unwillingness to believe she was affected with illness, or formed part of the disorder, I cannot take upon me to say; but certain it is that no less than four or five of these poor creatures have assured me, in language of gratitude and self-satisfaction, that they felt perfectly well, and this too when their general symptoms plainly forbade all hope of recovery.

This complete unconsciousness of danger, however remote, at a time when the hand of death was almost upon the patient, was a curious and distressing feature of the disease; and is the more remarkable from the fact, that these women were apparently in full and perfect possession of their mental faculties. I have once or twice before observed the same in women dying of pure metro-phlebitis.

Vomiting was not by any means a very prominent or constant symptom, except in the marked peritonitic cases; though in nearly all the fatal cases it came on some hours before death. Several of those who recovered had sickness of stomach, and a few of them even vomited large quantities of the dark-green tenacious fluid which has been aptly compared to green paint.

Guided by the experience of this epidemic, I feel disposed to regard the state of the tongue as a more reliable prognostic than any other *single* symptom. With only one or two partial exceptions, I never saw a patient recover when the tongue had become dry, or brown, or glazed; I have observed this symptom before any of the others had assumed a mortal or even threatening character; nor was it absent in any of the fatal cases of the disease.

At the outset of an attack the tongue was usually white, slightly furred, and somewhat less moist than natural. In many cases this state of the organ has been the very first symptom to excite alarm, and to apprize us of the coming storm.

As the disease made progress, the next unfavorable change observed in the state of the tongue was a dry, brownish streak down its centre, and more remarkable towards the base. This condition gradually extended until the entire dorsal surface of the organ was involved.

I think I am justified in asserting that the prevailing character of the tongue in the late epidemic was a close approximation to what is usually called the "typhoid tongue," and this is one symptom wherein it differed from the epidemic of 1845, in which the tongue presented most usually a broad, soft, creamy appearance. Mr. Hey, in his 'Account of the Puerperal Fever,' as it visited Leeds, makes the following remarks, which are pertinent to our present subject,



as marking the contrast, in this symptom, between the two epidemics:

"The tongue was never incrustated with the dry brown fur of typhus, except the disease was of long continuance, or had been improperly treated. It was generally moist and soft, and though it was not unfrequently covered with a thick white or brownish fur, yet it *was often but little altered from its natural appearance to the last, even in bad cases.*"

Diarrhœa was present in most of our cases, but was not so conspicuous or so formidable a complication as in the epidemic of 1845. I cannot but think that its first production was often attributable to the mercury and some of the other remedies which were used to subdue the disease; had it been otherwise, it is probable we should have experienced more difficulty in restraining it.

Fulness of the belly, with tympanitis to a greater or less extent, was almost universal, but in the individual cases this condition did not become remarkable till an advanced stage of the complaint, except in those which showed from an early period a preponderance of the symptoms referable to inflammation of the peritoneum.

The extreme rarity of cerebral disturbance in the course of puerperal fever is attested by nearly all observers, and the general tenor of my own experience agrees therewith. Nevertheless, I saw four cases which were exceptions to this rule.

There seemed to exist throughout the epidemic a strong tendency to putrescence or sloughing of the uterus and vagina, and this, too, quite irrespective of the length or character of the labour. In six cases we had direct proof of the existence of this gangrenous condition; two of these were patients that recovered and had sloughing of the vagina.

This constitutes an important feature in the late epidemic, and places it in strong contrast with the disease as it presented itself to Dr. Joseph Clarke and Dr. Collins; for neither of these authors make any mention of such having occurred in their experience.

It has already been stated that in every instance the pulse was found to be very rapid. At the commencement of an attack it was rarely below 112, occasionally much higher: and as the symptoms became more developed, and the disease made progress, the pulse commonly rose to 130, 140, and even 160. The other characters of the pulse were sufficiently remarkable to render them deserving of notice. In no one instance could we have applied to it the epithet "incompressible;" on the contrary, it was invariably soft and yielding, and gave to the finger a sensation that is best described by calling it "liquid or undulating."

During the epidemic of 1845, and I believe in former epidemics also, trismus and convulsions prevailed to an unusual extent among the children born in the hospital. It is a fact, however, worth recording, that not a single example of either of these complaints presented itself during the entire period of the late visitation.

This low, insidious manner in which the disorder not unfrequently crept into the system (if I may so say), taking hold upon the vitals without giving any unequivocal evidence of its presence, constitutes,

I think, a remarkable feature of the epidemic, and places it in strong contrast with the epidemics described by Gordon, Hey, Armstrong, Joseph Clarke, Collins, and others.

The treatment, as usual, was not very satisfactory. Dr. M'Clin-tock's plan was, "to leech promptly, to purge actively, and to stimulate freely."

ART. 124.—*The treatment of Puerperal Fever by large doses of Opium.*

By Dr. A. CLARK, Professor of Physiology and Pathology in the College of Physicians and Surgeons, New York.

(*New York Journal of Medicine*, March, 1855.)

Dr. Clark has appended to the new American edition of 'Ramsbotham's Midwifery' his experience in this mode of treatment at the Bellevue Hospital, New York. He relates one case, and refers to several others; and from these he thinks the following conclusions are justifiable:—

"1. When a prominent element in 'puerperal fever' is peritonitis, the treatment with large doses of opium is more successful than any other that has yet been proposed.

"2. To be successful, this treatment must be commenced early, and the patient must be brought under its influence as rapidly as the susceptibility of the system can be ascertained by trial.

"3. The quantity of opium required to produce a safe but desirable degree of narcotism, varies greatly in different cases; so that it is necessary to begin with doses that cannot do mischief, and increase every two hours till the influence of the opiate is sufficiently decided.

"4. Every dose, during at least the whole tentative period, should be administered by the physician himself, or by some person on whose knowledge of the effects of opium and whose watchfulness and discretion he can rely. Some young physicians are too bold, and endanger the life of the patient; others are too timid, and do not control the disease.

"5. The opium treatment alone will not cure 'puerperal fever,' when its leading element is purulent metritis, though there is reason to believe that it will control, and even prevent, the peritonitis which generally accompanies it. This conclusion has been confirmed by recent observations.

"6. The tolerance of opium in some cases of puerperal peritonitis almost surpasses belief. Yet in private practice I have not found more than half or two thirds of a grain of sulph. morph., every two hours, necessary, and have generally begun with less, except for the first dose.

"7. The influence of the opium should be kept up till the pain and tenderness subside, the tympanitis diminishes in some degree, and the pulse falls below 100—then with the concurrence of other symptoms, it should be gradually diminished, and at length discontinued.

"A few remarks and statements may be needed to make some of these conclusions intelligible.

"The usual effects of opium given in efficient doses for the cure of this disease are, a disposition to sleep, but not profoundly; a contracted pupil; perspiration, often profuse; sometimes a red, blotchy eruption; diminished frequency of the respiration; subsidence of pain and tenderness; slight suffusion of the eyes; and, after a variable time, reduced frequency of pulse. Of these effects, three have been chiefly regarded as the criterion by which each particular dose is to be governed. If, when a dose is due, the sleep be profound (the amount of sleep is of little importance, if the patient be easily roused from it), there is reason to hesitate; if the respiration has already been reduced to twelve in the minute, and is *very* irregular and sighing, the dose should be diminished or wholly withheld; yet so long as the tenderness continues, it is desirable to urge the opiate, but, of course, always within the limits of safety.

"The respiration appears to be the most certain indication of danger. I have not generally aimed to reduce it below twelve in the minute. Yet in almost every case it has fallen, once or twice in the course of the treatment, as low as seven, and sometimes to five. In no instance, however, has the narcotism, taken as a whole, been so profound as in the case detailed above. No instance of fatal narcotism has yet occurred under my observation, nor among the many cases reported to me by others.

"Regarding the tolerance of opiates in some of these cases—at the risk of being charged with rashness and trifling with human life, I will make some extracts from case 7. The treatment was commenced at 10 a.m., on the 26th of December—two grains of opium hourly. At 2 p.m., no change in symptoms, dose increased to gr. iv. At 3, gr. iv. At 4, gr. v. At 5, gr. v. At 6, gr. viij. At 8, gr. x. At 9, gr. xij. At 11, sol. morph. sulph. (sixteen gr. to fl. 3j), 3iss. At 12, 3j. At 1½ a.m. (respiration 6), 0. At 6 a.m. (respiration 12), opium, gr. xij. At 10, sol., 3j. At 12 m., opium, gr. xij. At 1½ p.m., sol., 3ij. At 2½, 3ij. At 3½, opium, xxiv. At 5, gr. xij. At 6½, sol., 3ijss. At 7½, 3ij. At 9, opium, gr. xiv. At 10, gr. xvj. At 11, gr. xvij. 28th, at 1 a.m., sol., 3ijss. At 2, 3iv. At 3¼, opium, gr. xx. At 4, sol., 3ijss. At 5, 3ijj. At 6, 3ijjss. At 6½, opium, gr. x. At 7, sol., 3ijjss. At 8, opium, gr. xxij. At 9½, sol., 3iv. At 10, 3ijj. At 11½, 3ijj. At 12, 0. Thus this woman took, in the first twenty-six hours of her treatment, opium, gr. lxxvij, and sulph. morph., gr. vij; or, counting one grain of sulph. morph. as four grains of opium, one hundred and six (106) grains of opium. In the second twenty-four hours, she took opium, gr. cxlvij, and sulph. morph., gr. lxxxj, or opium, four hundred and seventy-two (472) grains. On the third day, she took 236 grains. On the fourth, 120 grains. On the fifth, 54 grains. On the sixth, 22 grains. On the seventh day, 8 grains. After which, the treatment was wholly suspended. This woman was not addicted to drinking, and after her recovery, she assured me repeatedly that she did not know opium by sight, and had never taken it, or any of its preparations, unless it had been prescribed by a physician. This is, perhaps, 'horrible dosing,' and only justifiable as an experiment on a desperate disease. Yet this woman is alive to tell her story, as are several others who took surprising



quantities of this drug. But later observations have shown that the tenth to the twentieth part of this maximum is efficient in controlling the disease. So this case is referred to, not for imitation, but because, with similar cases, it is a medical curiosity; and may, perhaps, open some new therapeutical views.

“The results of the opium treatment, in the hands of my professional friends in this city, have not been uniformly successful. This was to have been expected. When the path to success is so narrow, and so little trodden, though beset with dangers on both sides, it is unavoidable that many will lose it. But, I believe, I am authorised in saying, that those who have seen most of this mode of medication, are most attached to it. It is not to be expected, that in a disease so dangerous as the one under consideration, any plan can be uniformly successful, even with advantages of accurate diagnosis and early treatment; but, when it is remembered that the diagnosis between purulent metritis and puerperal peritonitis is not always easy,—and that this medication is successful in proportion to its early adoption,—we may probably find reason for its failure in other hands, as well as in my own.

“By way of illustrating the vigilance and discretion which must be exercised in the administration of each successive dose of the opiate in this mode of treatment, I will add, that it could never have been fairly tested by me without the zealous, intelligent, and untiring assistance of the house-physicians of the hospital. They visited the patients every hour by night as well as by day, and every dose of the medicine, from the first case to the last, was given by them, and proportioned to the hourly exigencies.”

ART. 125.—*Cæsarian section twice successfully performed on the same patient.* By M. STOLZ.

(Gaz. Méd. de Paris, June 16 and 23, 1855.)

M. Stolz has performed the operation six times, with safety to both mother and child in four instances, and with safety to the child in the remaining two. The last of these cases is the one now recorded.

He also enumerates fourteen well authenticated cases in which this operation has been twice successfully performed on the same patient. In two of these cases the operation was performed *thrice*, the mother not surviving the third. In one the operation was performed four times, and with complete success.

Dr. Mangold, of Bale, operated, first in 1797, and again in 1801, and M. Mautz, in 1837, upon the same patient, who died on the twenty-second day after the third operation.

Dr. Bacqua, of Nantes, operated successfully on one patient, first in 1797, and again in 1806.

Dr. Dariste, of Martinique, operated successfully on one patient in 1805 and 1807.

Dr. Lemaistre d'Aix, operated three times on one patient, in 1805, 1807, and 1814. The patient died five days after the last operation.

Dr. Charmeil operated in 1813 (?), and another surgeon in 1814, on one patient, each successfully.

Chaussier communicates the successful performance of the operation a second time.

Merrem of Cologne, operated successfully on one patient, first in 1821, and again in 1826. Dr. Zwanck operated for the first time on a patient named Adametz; Dr. Weidemann operated a second time; Dr. Michaelis a third, and again a fourth time, with success.

Dr. Rouvin communicates a case where the operation was repeated with success. Dr. Bowen performed the operation twice on one patient, in 1833 and 1835, with success.

Professor Kilian of Bonn, operated successfully on one patient, first in 1832, and then in 1838. In another case, he operated in 1837, and the same patient recovered from a repetition of the operation in 1843.

Dr. Mestenhaeuser operated successfully on one patient in 1840 and 1844.

CASE.—The patient, Adèle Fenninger, æt. 39, was the subject of general rachitic deformity, and had been operated on, three years previously, by Dr. Bach, of Strashourg, who published the case in the 'Gazette Médicale' of that city, in 1846. On that occasion the operation was successful, although performed under very unfavorable circumstances, the patient being "radicalement" rachitic, in very delicate health, ill nourished, and suffering from considerable obliquity of the uterus. At the time of the operation, somewhat too large an incision in the abdominal and uterine walls was attended with troublesome protrusion of the intestines; and during the progress of the case, erysipelatous inflammation of the points of suture, and phlebitis, followed by œdema of the left inferior extremity, were circumstances which more than once threatened the life of the patient; however, by careful attention, these dangers were averted, and a complete cure was obtained in the space of two and a half months.

The second operation, which presents no features of much importance, is minutely described by M. Stolz, who remarks, that from the ordinary course of the second pregnancy, three years after the first operation, it may be concluded that the cicatrization of the uterus had been sufficiently perfect to resist with impunity a second dilatation of that organ; and that the second operation, like the first, had been complete in its success, mother and child being both saved.

M. Stolz argues from these facts that the operation cannot be so serious in its consequences as is generally supposed.

ART. 126.—*Extrusion of the Eyeball in the child during birth in two successive labours.* By M. HOFMAN, of Burgsteinfurt.

(*Monatschr. für Geburt.*, Dec. 1854; and *Edinburgh Medical Journal*, Oct., 1855.)

This extraordinary occurrence is deserving of attention, not only on account of its rarity, but also in a medico-legal point of view, as being calculated to arouse suspicions of the culpability of those in attendance on, or connected with, such cases, if not to lead to their criminal accusation.

CASE.—M. Hofman was hastily summoned as medical attendant, and on

his arrival found the child born, and in a bath. The infant was a girl, at the full period, strong, and well formed. The right eye was hanging out of the orbit, upon the cheek, attached by a few muscular fibres, and surrounded by some loose cellular tissue. About three quarters of an inch of the optic nerve was still attached to the eye, and the ophthalmic vein and artery were torn off close to it. The cornea was bright and transparent, the eyelids were swollen and congested, and the orbit was filled with a grumous fleshy-looking mass. The child did not appear to suffer much, and the head, which was of normal dimensions, showed no depression on any part. The extruded eye was removed, and in a few weeks the eyelid, under appropriate treatment, recovered from the effects of the injury.

The same woman, having again become pregnant, again came under the care of M. Hofman, who found that labour proceeded slowly, and that considerable difficulty accompanied the delivery. The infant on this occasion was a boy, robust and well formed, and the head of the usual size. As in the last case, the right eye was extruded, and totally detached from the socket, which was still bleeding; the eyelids were neither red nor swollen, as in the last occasion; the orbit was filled with coagulated blood, and, above it, there was a fracture with considerable depression of the frontal bone. The child cried incessantly, and died the same day.

A third pregnancy, in which premature labour was induced, and which terminated fatally for the mother, afforded an opportunity of ascertaining, by post-mortem examination, the cause of these remarkable injuries in the two former accouchements. The sacro-pubic diameter was only three inches, and the promontory of the sacrum projected very much. The transverse diameter was  $4\frac{3}{4}$ , and the oblique  $4\frac{1}{2}$  inches. No obliquity or exostosis existed in the pelvis, and the vertebral column was straight; the symphysis pubis was somewhat separated.

There seems no doubt that the depressed fracture of the frontal bone was produced by the promontory, and that both cases were merely different degrees of the same effect, arising from the same cause. The compression, exerted through the frontal bone upon the roof of the orbit, produced expulsion of the eyeball to a certain degree, and the uterine contractions continuing to go on, the eye, already protruded from its socket, became arrested by the prominence of the sacrum, and was dragged and torn from its connexions during delivery.

ART. 127.—*Case of quadruple birth.*  
By Dr. WM. RANKIN, of Skippensburg.

(*American Quarterly Journal of Medical Science*, April, 1855.)

This case is told as follows :—

April 20, 1854. I was called to visit the wife of Mr. John Tarvin, farmer, who was supposed to be in labour, having completed the full term of her fourth pregnancy. On my arrival, I found a child delivered; but, on examination, ascertained that there was another contained in the uterus. After waiting nearly an hour from the time the first child was born, for further uterine contractions, I ruptured a set of membranes that presented in the form of a pouch at the os uteri, when a pain came on and delivered another child. On examination over the region of the uterus, I discovered the presence of another child; and no pain coming on, after a reasonable delay, I ruptured a third set of membranes, when a powerfully expulsive contraction took place and ex-



pelled a third child. From the usual examination, I ascertained that there was yet another child to be delivered; and no pains coming on, I resorted to the plan of rupturing the membranes, when almost immediately a pain occurred and effected the delivery of a fourth child. By a little manipulation over the region of the uterus it contracted and expelled from its cavity two placentaë, to one of which three cords were attached, and to the other but one; the whole mass not larger, I think, than I have often seen after the delivery of one child. The cords were something smaller than the average size. The uterus afterwards contracted well, and there was no more hemorrhage than takes place after the majority of cases of parturition. The woman appeared very much exhausted and overcome by distracting emotions in consequence of the enormous brood of children to which she had just given birth; but after encouraging and consoling her, as well as I could, by pointing out her duty in the case, and assuring her that there would, undoubtedly, be ample means provided for their maintenance, she soon assumed a cheerful countenance, her general debility gradually abated, and she progressed through her confinement as favorably as most women do after giving birth to a single child.

The first and last born were rather larger than their mates, and were all apparently healthy and viable, and, according to their mother's reckoning, had come to their full time. Although I unfortunately neglected weighing them, I think they would have averaged four or five pounds.

They were all of the male sex. The hair nearly red, and the complexion florid in all of them.

The mother is a woman of slender and rather delicate form, of medium height, and generally healthy, of about thirty-five years old; and, before this quadruple birth, had had three daughters at as many births; five years have elapsed since the last. Her complexion and hair are dark. The father is a stout, athletic, healthy man, of about thirty-eight years of age; hair fair and complexion florid; of sanguine temperament.

The first born of the children, about eight weeks after birth, sickened and died after a few hours' illness, most probably from the almost unavoidable irregularity in giving it nourishment.

After the milk was fully secreted, the mother had, for a month or so, plenty of nourishment for them all, after which time food was required from other sources, principally cow's milk diluted with water, and sweetened with loaf sugar. The three surviving are now, 14th of January, 1855, quite healthy and thriving children.

ART. 128.—*Synopsis of thirty cases of Ovariectomy occurring in the practice of the Author.* By Dr. WASHINGTON ATLEE, of Philadelphia.

(*American Journal of Medical Science*, April, 1855.)

In this paper we have short summaries of all the cases in which Dr. Atlee has performed this operation. Some of the cases, however, must be deducted, for in them nothing more was done than to make an exploratory incision. Deducting these, twenty-six cases remain, of which twenty-two are cases of ovarian, and four of uterine disease.

Of the ovarian cases, eleven deaths are recorded and eleven recoveries. Of the uterine, two deaths and two recoveries.

Of the thirteen recoveries, ten are now alive—five of whom were operated on between March 1844 and January 1852—one in

September, 1853, and four between April and December, 1854—and three are dead; one dying thirty-nine days after an attack of cholera, from eating heartily of duck, &c.; one thirty days after, of starvation, caused by excessive irritability of stomach, the consequence of pregnancy; (Dr. Atlee asks whether the production of abortion, the patient being two months gone, might not have preserved life in this case;) and one from phthisis pulmonalis three years after the operation.

The condition of health of the ten now living, a most important consideration, is not noted, excepting in one instance, where it is represented as being perfect. Two cases have been pregnant, but miscarried, and one has given birth to two large, healthy children, since the operation.

In the thirteen which died, death was caused by peritonitis in four cases, by exhaustion in five, by hemorrhage in one, secondary hemorrhage in two, and by gangrenous perforation of the jejunum in one.

The period of death, after operation, was nine hours in one case; thirteen hours in one case; on the third day in five cases; on the fifth day in two cases; on the sixth in three cases; and on the twenty-second day in one case.

The average time of death of these cases was about five days. The operation was performed with the hope of arresting impending death in seven cases, four of which terminated fatally.

Of the twenty-six cases, in all of which extirpation was performed, 15 or 57 per cent. of the whole number died within forty days.

Of the eleven remaining cases, one died of phthisis pulmonalis, an three years after operation, and ten were alive up to the period of publication of Dr. Atlee's paper.

The condition of health of those still living is not noted, excepting in one instance.

With these results, it really becomes a serious question whether the operation is justifiable, for we can scarcely suppose that the operation will be more successful in the hands of other surgeons. At any rate, it is not likely that the surgeons will have greater practical acquaintance with the operation.

ART. 129.—*The hereditariness of Ovarian disease.* By Dr. LEVER.

(*Guy's Hospital Reports*, 1855.)

Dr. Lever adduces the fact of six deaths and one impending death, in the members of one family, as a most conclusive proof of the hereditariness of this affection, as indeed it is:

“ M. F., died at the age of 79 years.

M. A.,        “        “        77        “

M. S.,        “        “        48        “

E. S.,        “        “        28        “        inspected by Mr. Callaway.

M. D.,        “        “        49        “

E. D.,        “        “        20        “        operated on unsuccessfully by Mr. Key,  
in 1843.

M. S., an outpatient at Guy's, who ere long will die.”

ART. 130.—*Rupture of the Perineum and recto-vaginal Fistula treated by cauterization.* By M. CLOQUET.

(*Archiv. Gén. de Méd.*, Feb., 1855.)

In a memoir submitted to the Academy of Sciences at Paris, M. Cloquet states that he has successfully applied the treatment recommended by him in cases of split palate ('Abstract,' xxi, p. 305) to cases of ruptured perineum and recto-vaginal fistula. If the treatment be persevered in, he says, the edges of the unnatural opening are sure to unite. The memoir contains several cases, which show the comparative rapidity, as well as the completeness, of this mode of operating.

ART. 131.—*Recto-vaginal Fistula treated as fistula-in-ano.*  
By Mr. COCK.

(*Guy's Hospital Reports*, 1855.)

This case was very analogous to fistula-in-ano, and it was treated accordingly, that is, by the complete division of the tissues intervening between it and the external surface. The only objection which appears to apply to this operation is the fear that the cicatrized perineum may not be equal to bear the strain of future confinements, but this objection, in Dr. Lever's opinion, is of no great weight.

CASE.—S. P——, æt. 37, was admitted on September 20th, 1853, under the care of Dr. Lever.

It appears that, fourteen months previous to her admission, she had a lingering labour of her fourth child, and that ever since that period she has been inconvenienced by the discharge of fæces, and the passage of flatus, per vaginam. She had been treated as an out-patient for some time, chiefly on account of leucorrhœa, and the treatment for this was first prescribed on her admission; it consisted in the administration of Julep. Magnesiae, ter die, with an occasional purgative of Pulv. Rhei Salinæ, and the use of Decoct. Tormentillæ pro Injectione. Under this, and the use of quinine, she was relieved of her leucorrhœa, and attention was then directed to the fistula, with a view to operation. On examination, it was found that a fistulous opening existed in the vagina, and was felt by the finger about an inch beyond the fourchette, although not easily detected, as it was hidden by a fold of mucous membrane. On the introduction of another finger into the rectum, by a little manipulation, the tip of its fellow could be reached.

On the 10th of October, Mr. Poland determined to perform an operation. A probe was introduced through the opening in the vagina, and it was found to pass in an upward direction in the recto-vaginal septum before it communicated with the bowel. When it reached here, the end of it was seized, and brought out at the anus. The probe thus served as a guide for the bistoury, which was introduced by its side into the rectum from the vagina, and the point being there met by the forefinger of the left hand, the operation was completed in the manner of that for fistula in ano, the whole of the tissues between the rectum and vagina being divided in the median line of the perineum. But slight hemorrhage followed; the edges of the wound were kept asunder by oiled lint, and the patient was put to bed. From this



period to the 27th of the same month, the patient's bowels, habitually costive, did not act, and as she was somewhat feverish, Dr. Lever ordered her *Mist. Oleosæ c. Rheo, Ziss*, which operated effectually.

The lint was withdrawn gradually,—*i. e.*, was introduced less deeply at each dressing,—and the wound allowed to heal from the bottom.

Perfect cicatrization not ensuing (notwithstanding the use of [black wash]) as rapidly as could be wished, it was thought better for her to leave the hospital for change of air, and she was accordingly presented on the 22d of November.

Subsequent accounts of the patient informed us that in a few days the wound had completely healed, and that no further inconvenience had been felt.

I have seen this patient subsequently, and she is perfectly well.

ART. 132.—*On the origin of retro-uterine Hematocele.*

By M. LAUGIER.

(*Gaz. Méd. de Paris*, Mars 10, 1855.)

M. Laugier read a communication on this subject at the meeting of the Academy of Sciences of February 26. After adverting to the imperfect state of our knowledge on this disease, he entered at some length into several interesting questions connected with it, and concluded with the following remarks as the result of his investigations:

1. The spontaneous evolution of the ovule is, as has been alleged, an occasional cause of retro-uterine hematocele.

2. The physiological state of congestion in the ovaries during this spontaneous evolution, and the persistence of the opening in the Graafian vesicle, do not occasion retro-uterine hematocele.

3. To produce this there must exist an increased degree of congestion, sometimes occasioned by accidental causes, during, or a few days after, menstruation. Abortion is not an immediate cause of hematocele, as has been erroneously supposed.

4. It is especially the recurrence of this spontaneous evolution which gradually increases the volume of the hematocele.

5. The ovarian vesicles successively opening into the cyst of the hematocele remain open there, so that the ovary is destroyed by a small number of spontaneous evolutions taking place in the condition which that organ presents at the commencement of hematocele.

6. The rupture of a Graafian vesicle affording a passage for the blood which escapes from the ovary, the cyst of the hematocele will be most frequently intra-peritoneal.

7. Spontaneous evolution of the ovule and hematocele have one character in common, namely, pain situated in one side of the abdomen, and the seat of which is the ovary where the vascular evolution occurs.

8. The rut may occasion ovarian congestion in animals, and may be followed by rupture of that organ, that is to say, by consequences simulating retro-uterine hematocele.

ART. 133.—*Removal of the entire body of the Uterus by the large abdominal section.* By Dr. E. R. PEASLEE.

(*American Journal of Medical Science*, May, 1855.)

The following is a short abstract of this case :

CASE.—A widow, æt. 35, and mother of four children. Death happened on the fifth day after the operation, from gangrene produced by strangulation of two hernial protrusions. The operation was undertaken for the removal of an ovarian tumour, the diagnosis of which was, to all appearances, very clearly made out. The mistake in the diagnosis, which the subsequent steps of the operation showed, was most certainly not a culpable one. On examination the tumour was found to be a fibrous growth, already softening and degenerating in its central portions. "It must have originated at the right side of the fundus of the uterus, and in its substance, though near its surface. Death, which occurred on the fifth day after the operation, appears to have been produced, not from the removal of the uterus, but from the complications of two hernial protrusions,—a small one near the lower end of the incision, and a larger near the upper end—produced by coughing and retching. These hernial protrusions had occurred the morning after the operation, in the absence of Dr. Peaslee; and when examined, they were livid, and covered with plastic exudation. A portion of the sutures were removed, and they were returned. The post-mortem appearances went to show that the gangrene extended from the portion of the bowels implicated in the herniæ, and thence to the peritoneum.

ART. 134.—*Sequel to a case of extirpation of the Uterus.*  
By Mr. WINDSOR, Surgeon to the Manchester Eye Hospital.

(*Lancet*, May 19, 1855.)

The operation in this case was performed August 22d, 1818, and an account of the case was communicated to the Royal Medical and Chirurgical Society on June 22d, 1819, and printed in the tenth volume of the Society's 'Transactions.' The patient died from an accident, October 27th, 1854, aged sixty-eight years. Her age at the time of the operation was thirty-one. For the first ten years she had irregular discharges of coagula at intervals varying from two to six months; these intervals gradually became much longer, and all discharge of blood ceased about the age of fifty. The following is the author's account of the state of the parts after their removal from the body:—"The preparation shows well the os uteri apparently in its normal state; it is about half an inch in width. A probe passed through it into the blind or closed cavity beyond does not penetrate more than three-eighths of an inch. This therefore is all that remains of the cervix uteri by the operation performed in 1818. The communication with the abdomen seems to have closed well. Under the abdominal aspect of the cervix (or opposite to the vaginal one) a sort of extended membranous or fleshy surface is seen, on which a portion, apparently, of one Fallopian tube, with its fimbriated extremity, can be traced; and near it is an appearance of an atrophied ovarium. On

the opposite side are somewhat similar appearances of tube and ovarium. Both terminate in the membranous-like expansion near to the cervix uteri, each being about two inches in length." This case is rendered additionally interesting from the fact that the woman was four times the subject of strangulated hernia on the right side; that on the first occasion, no surgical treatment being permitted, the tumour sloughed on the eighth day, fæces were discharged from the wound, and in six weeks the opening spontaneously closed. This occurred about 1840. In 1850 she was twice operated on, at intervals of six weeks; and in 1853, the fourth occasion, the hernia was returned by the taxis with some difficulty. Her death was the result of a severe injury to the head, occasioned by an accident while travelling.

ART. 135.—*On the high operation for Stone in the Female.* By Dr. W. PARKER, Professor of Surgery in the College of Physicians and Surgeons of New York.

(*New York Journal of Medicine*, March, 1855.)

After some remarks upon the history of this operation, Dr. Parker relates three cases, which comprise his experience in the matter. These cases we subjoin. Dr. Parker considers this operation to be safer than any yet pursued in females, particularly where the precaution is taken of hooking up the walls of the bladder, before laying the viscus open, so as to prevent urinary regurgitation or infiltration.

CASE 1.—Mrs. L—, æt. 53, married, and the mother of ten children, had always resided in the interior of Ohio, and had invariably enjoyed good health until about four years previously to the time of her coming under my observation. Her first complaint was of suffering in passing water, the symptoms being those of urinary calculus. The physician in attendance had examined the bladder with the sound, and detected the presence of a stone. I was called in consultation, with special reference to the operation. As the patient resided some 80 miles distant from me, and her sufferings were becoming more and more aggravating, there seemed no alternative but the knife.

The stone was large, and I determined to perform the high operation. The operation was commenced by injecting the bladder with warm water. To accomplish this, the patient was placed upon the table in the proper position for lithotomy, and the fluid being slowly injected, an assistant pressed the urethra firmly against the arch of the pubes, to prevent its escape. This preparatory step having been taken, I proceeded to make an incision above the pubes, and extended it two and a half inches along the linea alba, cutting down to the bladder. I now opened the bladder with a pointed bistoury, which was followed by such a regurgitation of fluid through the artificial passage, that the assistant was directed to remove his finger from the urethra, and allow the contents of the bladder to escape. I then introduced the *brise-pierre* of Baron Heurteloupe into the bladder, through the urethra, and separating the blades, I raised the fundus towards the external opening, until I was able to reach it with the tenaculum. With this instrument, and the aid of an assistant, I kept the bladder firmly drawn up to the external opening. The incision not being sufficiently free, was now enlarged,



and the finger being introduced, the forceps were readily carried into the bladder, and the stone seized and removed with great ease.

The operation was terminated by closing the upper portion of the wound, and leaving the lower part open to admit of the free escape of urine or pus, and thus prevent infiltration. The patient recovered rapidly. The stone was nearly two inches in length, and one and a half in breadth.

CASE 2.—Miss M—, æt. 53, living in the country, consulted me for symptoms of calculus, which had existed for the last seven years. On examination, I found a large stone. The urine contained pus, and epithelial scales from the bladder; regarding this organ, however, sufficiently sound for the trial of lithotripsy, I proceeded to prepare her system for this operation, by vegetable diet, demulcent drinks, and attention to the bowels.

The operation was commenced by the usual injection of warm water. I then introduced the lithotripter, and seized the stone, but could not break it by the force of the hand. With the hammer, I succeeded in breaking off about one drachm, in small pieces. These were proved to be oxalate of lime, on applying proper tests. These attempts were repeated up to the ninth day, but with no success; and in the mean time the bladder had become inflamed with considerable constitutional disturbance. She was now kept quiet in bed, as all motion was attended by much pain.

In about three weeks the system became tranquil, and I proceeded to remove the stone by the high operation. The following were the steps of the operation which I performed :

*First.*—The patient was put fully under the influence of chloroform,

*Secondly.*—The bladder was injected with flax-seed tea, and owing to the system being under the influence of the anesthetic, the injection was introduced without any resistance, and the organ well filled.

*Thirdly.*—An assistant prevented the escape of the fluid, by pressing the urethra against the pubic arch.

*Fourthly.*—The pubes being well shaved, I made an incision along the linea alba, through the fat and muscles, until I reached the bladder.

*Fifthly.*—I now hooked strongly through the bladder, by large tenacula, and gave them in charge of assistants. I then punctured the bladder with a pointed bistoury, and made an incision about one and a half inch in length. No regurgitation took place. Upon my finger I introduced the ordinary nasal polypus forceps, and easily removed the stone.

*Sixthly.*—The assistant now removed his finger from the urethra, the fluid escaped, and the hooks were removed.

The upper part of the wound was closed with a suture and straps, the lower portion being left open for the escape of any discharge. The operation was performed on the 12th of October, 1853, and on the 22d she was able to sit up. The result was very satisfactory.

CASE 3.—Patient was Mrs. E—, æt. 47, a widow. This lady had suffered from a stone in the bladder, until she could hardly move about, and at the time she came under my observation, her sufferings were intense. The urine was alkaline, and loaded with pus and blood.

I ascertained that I could do nothing with the lithotripter, and accordingly advised an operation as the only hope of relief. She at length made up her mind to submit to the operation, and sent for me for the purpose of having it performed.

On the 25th of August, 1854, I operated according to the plan laid down in the preceding case, and notwithstanding the amount of disease of the bladder, the patient improved greatly, and three months after she was comfortable, but had a slight opening above the pubes, from which pus at times escaped.

ART. 136.—*Case of Stearrhœa Nigricans.* By Dr. NELIGAN.

(Assoc. Med. Journal, May 18, 1855.)

In the 'Dublin Quarterly Journal of Medical Science' for May, 1855, Dr. J. M. Neligan has published an essay on black discoloration of the skin of the face. The affection (at least in the intense form described by Dr. Neligan) is rare, but some examples have been well described, especially by the late Mr. Teevan, in vol. xxviii of the 'Med.-Chir. Transactions,' and Dr. Yonge, of Plymouth, in the 'Philosophical Transactions,' in the year 1709.

The history of the case related by Dr. Neligan was communicated to him by Dr. Quinan, physician to the Donnybrook Dispensary. The following is an abstract:

CASE.—E. D——, æt. 21, unmarried, a dressmaker, of leucophlegmatic habit, had been in good health till about the age of 19, when the catamenia, which had been regular, ceased altogether. Shortly afterwards, a large patch of erysipelatous redness appeared on the right side of the body, and soon disappeared; and it reappeared and disappeared (its duration being prolonged each time) at each monthly period. Tartar emetic in small doses was prescribed by a medical man, under the idea that the redness was from erysipelas. She, however, had a fearful attack of nausea and vomiting, and had since been subject to sickness of the stomach, occurring regularly half an hour after the first meal in the morning, followed by vomiting of food. She had a troublesome cough, with loss of appetite and of strength.

In August last, she went to England to try the effect of a short sea voyage and change of air; but she was there told by a medical gentleman that she was threatened with consumption, and she returned home. At this time the erysipelatous redness ceased to appear, though the catamenia were still absent; and she perceived, after a fit of retching, some blood in her expectoration.

At the next monthly return (September) the symptoms were more violent; and she threw up about half a pint of reddish-brown matter every morning for four or five days.

In October the vomiting returned; and now, for the first time, a dark, bluish-black stain appeared at the inner canthus of the left eye. When Dr. Quinan saw her, there was a large patch under each eye; the skin was tender; and the vomiting continued every morning. The treatment employed consisted in the administration of nourishing diet, tonics, emmenagogues, and stimulants, with the free use of open air exercise.

Dr. Neligan saw the patient in December, 1854. The girl was much emaciated, except in the face, and highly hysterical. The dark stains now covered nearly the whole of the right upper eyelid, and part of the left; both under eyelids were completely stained, and the dark patch extended on the right cheek. The colour was precisely that which would be produced by Indian ink, and rather set-off, or even gave additional beauty to a pleasing face. On examining with a lens, it was evident that the stain was dotted over the skin, corresponding to the sebaceous glands. No attempt was made to remove the stain by washing, on account of the pain which she had found the process to induce, and because the surface of the skin was exquisitely tender.

In April, 1855, Dr. Neligan again saw the patient. The general health was more broken down, although the cough and vomiting of dark matter had

diminished. The dark stains had become deeper in colour, and had extended below the malar bones and on the alæ of the nose. On the skin, at the confines of the stains, an exudation of the yellow matter of the sebaceous glands had taken place, precisely similar to what occurs in *steorrhæa flavescens*; and wherever this sebaceous fluid had been rubbed off, or the subjacent parts irritated, the integuments were inflamed, the sebaceous follicles hypertrophied, and their orifices enlarged, and in some cases filled with black matter.

Dr. Neligan also relates the history of a case communicated to him by Professor Law, of the College of Physicians in Ireland, occurring in a lady who consulted him twelve [three?] years ago. This case agreed with that of Dr. Quinan in the irritability of stomach, perverted appetite, and uterine derangement; but in Professor Law's case there was no pain, though there was some aversion to light.

All the cases of black discoloration of the face which have been recorded, Dr. Neligan states, occurred in young females affected with derangement, or partial or total suppression of the catamenia: in three—those most accurately reported—there was black vomiting; and in one, at least, the black colour disappeared with the restoration of the uterine function. Dr. Neligan terms the disease *steorrhæa nigricans*; and considers that, as not uncommonly occurs in females in whom the menstrual function is deranged, the sebaceous secretion is augmented in quantity, and in some cases is stained with the colouring matter of the blood—this being analogous to black-coloured vomiting, dark sputa, dark urine, or hemorrhagic subcutaneous extravasation. In the treatment, the evident indication is to restore a healthy state to the uterine function.

#### ART. 137.—*Case of vicarious secretion of Milk.*

By Dr. S. WEIR MITCHELL.

(*American Journal of Medical Science*, July, 1855.)

This curious case is thus related :

In the early part of July, 1853, Mrs. C—, æt. 20, was delivered of a child. The supply of milk proved scanty. Four weeks after delivery, a large mammary abscess formed in the left breast. This was opened, and, under treatment, became well in the course of three months. During this period, she nursed her child with the right breast. As the milk continued to form, though in small amount, in the left breast, the child finally obtained milk from both mammæ. At the close of a year, the mother became feeble, and subject to constant bronchial irritation. Her cough increased to such a degree that, for obvious reasons, she was directed to wean her child. The change was effected too suddenly, and she was told, accordingly, to allow her babe a part, at least of her own milk. In the interval of two days, the breasts had become swollen, and excessively painful. It was now found that the child could obtain no nutriment from this source, and that even the pump failed to empty the mammæ. Purgatives were ordered, and a water-dressing locally applied. Next day I found the breasts less painful, while the cough had become dry, hard, and almost constant. On the succeeding morning, she was greatly relieved by a loose cough, which enabled her to expectorate a quantity of white, firm sputa. I was told that Mrs. C. was spitting up milk,



and the white substance in question was exhibited in confirmation of the statement. During the day, upwards of a teacupful was thus cast up. This strange formation continued for a fortnight, to the manifest relief of hitherto urgent symptoms of declining health. My visits were made at irregular intervals, and for a time my patient was closely watched by the family; but, as the secretion was persistent in amount for a time, and as it was often coughed up in my presence, I see no reason to suspect collusion or hysterical deception. At the end of a fortnight the milky sputa became more pale, and at length gave place to mere mucus.

On the third day, and at intervals afterwards, I collected portions of the sputa, and submitted them to rigid examinations.

I found the discharge to consist of white clots, floating in a thick fluid, also of a white hue, and mingled with ordinary mucus. Very often a clot of yellowish muco-pus was enveloped in a covering of thickened milk. It is to be noted that the milk was, for the most part, in this clotted condition, and possessed the lacteal odour to a greater degree than the ordinary secretion itself.

Microscopic analysis revealed the presence of very perfect milk globules, mingled with compound granular cells, mucous corpuscles, and epithelia. I evaporated about one ounce and a half of the fluid, and, by repeated treatment with ether, collected a small amount of fat or oily matters. Nitric acid was found to coagulate the filtered fluid, whilst acetic acid produced but a slight cloud. Lastly, I was told by the patient that the sputa tasted like milk.

Mrs. C. was delivered of her second child in February of the present year. Her cough never left her, and at the period last alluded to, she was far advanced in phthisis. Her breasts were well filled, but her extreme feebleness obliged her to resign her child to a wet nurse. The breasts were artificially emptied, aperients employed, and no signs of her former trouble appeared until seven days after birth. At this time, for some forty hours, she coughed up a thin white fluid, mixed with the pus from a tuberculous cavity in the right lung. At the close of the period of time just mentioned, the pulmonary sputa regained its colour. I examined this specimen with the microscope only. It was thin, and small in quantity, but did not otherwise differ from the specimens obtained during the last year's illness. Both alike contained milk globules. The recurrence of the milky sputa was first observed by me, and pointed out to the patient. This, with the other facts above alluded to, induce me to regard the above stated case as indisputably one of vicarious or metastatic secretion of milk. We are thus called upon to admit that some part of the bronchial surfaces *may* repeat the role which nature has assigned to the mammary gland.

It may be as well to add that, in stating a case of interest mainly to the physiologist, I have avoided all detail as to the remedial measures employed from time to time.

#### (C) CONCERNING THE DISEASES OF CHILDREN.

ART. 138.—*On the treatment of Croup by large doses of Tartar Emetic.*  
By M. BAIZEAU.

(*Edinburgh Monthly Journal*, May, 1855.)

The frequency and fatality of this disease, confer an additional value on any remarks suggestive of improvement in the measures adopted for its cure. With this view the subject appears to have

been more fully investigated in France than in this country, and several important innovations in the method of treatment have originated there, as the result of this system of inquiry and observation.

Among these the employment of tartar emetic in large doses was suggested, as M. Baizeau allows, so far back as 1839, by M. Bazin; the same remedy was also proposed, but not employed by Laennec; and in the *Dictionnaire des Dictionnaires*, under the article *Croup*, we find it recommended by M. Fabre. Notwithstanding the opinion of such authorities regarding its value as a remedy in this disease, the matter seems to have been neglected; and it is with a view to reviving this mode of treatment that M. Baizeau has published his interesting communication in the *Gazette Médicale* for March 10th. Several cases are there cited by him as examples of the successful treatment of the disease by this method, and others are referred to where similar results have been obtained.

The success attending the employment of tartar emetic in croup, M. Baizeau ascribes to the contro-stimulant action of this substance, and not to its effects as an emetic, believing that its influence here is somewhat analogous to the apparently specific power exerted by it in other diseased conditions of the respiratory organs.

However, the vomiting necessarily arising from such doses as are recommended by M. Baizeau, can scarcely but be considered as materially assisting, if not in some cases superseding, the other effects of this remedy; the dose in different instances varying from six to twelve grains of tartar emetic in four ounces of water, and a teaspoonful of this to be given every two hours.

In combination with ipecacuanha, it is the emetic usually employed in the Hôpital des Enfants Malades at Paris, and only in those cases where it fails in producing its effects are others resorted to; the object being, that whatever emetic may be employed, the vomiting arising from it should be energetic and repeated, as would result from the doses given by M. Baizeau. And in support of this doctrine, M. Valleix has shown that out of fifty-three cases of croup, only one cure resulted in twenty-two of these cases where emetics were given sparingly; while fifteen recovered out of thirty-one cases in which they were administered more freely.

M. Baizeau, however, asserts that the absorption of this remedy into the circulation subsequently to the cessation of vomiting, and the fact that its contro-stimulant properties are thus brought into operation, afford the true explanation of its success. For, says our author, it generally becomes more difficult to excite vomiting in children after a few successive doses of any emetic, and thus as the remedy is longer retained by the stomach, the chances of its absorption are increased, and in this case its contro-stimulant action consequently becomes more active.

We do not clearly see, however, the manner in which M. Baizeau reconciles this theory with such statements as that of Barrier, who mentions that from numerous facts it is proved that emetics, among which he mentions tartar emetic, are efficacious only in those cases where they are followed by vigorous and continued vomiting; or with

the twenty-two cases of M. Valleix, quoted above, in which the vomiting was mild, and in which only one recovery took place. This apparent discrepancy, however, is at once removed, if we recognise the efficiency of tartar emetic as an emetic properly so called, and as at the same time, a powerful contro-stimulant, the combined actions mutually contributing to account for its success. In this way, along with its rapidity of action, and apparently specific powers, its superiority in such cases would be immediately apparent, as possessing advantages which are not to be found in any other remedy.

Although M. Baizeau has thus attached, perhaps, rather little importance to the occurrence of the vomiting produced in the first instance by this substance, his paper in other respects is a good one. And while the number of cases treated in this manner has been too small to justify any decided general opinion as to its merits, at the same time many facts in connection with these cases, would at least induce us to place more confidence in this remedy, and encourage its further trial by medical men.

ARE. 139.—*Tracheotomy in Childhood.* By M. GUERSANT.

(*L'Union Médicale*, Jan. 16, 1855.)

In this paper M. Guersant calls attention to the greater frequency with which he has been called upon to perform this operation at the Hôpital des Enfants Malades. Thus, in 1850, there were 10 operations; in 1851, 25; in 1852, 30; and in 1853, 60. M. Guersant concludes from these facts that croup is on the increase in Paris; a conclusion doubted by M. Denonvilliers, who ascribes these increasing numbers to the greater confidence of the public in the resources of surgery in such cases. As regards the success of the operation, M. Guersant remarks that out of 161 cases in which it was performed there were 36 cures; and it must be borne in mind that it was only performed as a last resource when asphyxia was considered to be imminent. The rest of the treatment was much the same as is usual in this country. M. Guersant also frequently employed cauterization of the larynx, but considers that this treatment does not deserve the credit it has attained.

M. Archambault directs attention to a peculiar phenomenon which frequently attends on recovery in those cases of croup in which tracheotomy is performed. As soon as the glottis is free from false membranes, it is observed that the infant, in swallowing, is extremely apt to be distressed by the passage of fluid matters into the larynx. This difficulty of deglutition has occurred in the practice both of M. Trousseau and of M. Guersant. The former attempts to remove it by giving only solid or semi-solid food; the latter proposes in extreme cases to pass the œsophagus-tube. M. Archambault recommends a simpler method. He removes the canula, and applies the thumb very firmly over the opening. When the child has made several respirations, and these have become regular and natural, he allows it to drink, after directing its attention to the necessity of caution. In this way drink can be taken. M. Archambault considers that this difficulty of deglu-



tition depends on an impaired sensibility of the glottis, and a want of harmony between the act of deglutition and respiration, owing to the artificial manner in which the latter has been effected through the canula. He has not witnessed this symptom except when the respiration was notably accelerated.

ART. 140.—*On incontinence of Urine in Children.*

By Dr. D. D. SLADE.

(*American Quarterly Journal of Med. Science*, July, 1855.)

Dr. Slade, following M. Trousseau, has given belladonna in several of these cases, and with the most satisfactory results. "So far as our experience goes," he says, "we have derived very satisfactory results from the use of belladonna in several cases of nocturnal incontinence." But as Trousseau explicitly says, "it did not have the same good effects in those cases where the enuresis occurred during the day, although it greatly relieved the trouble. We have, however, found that a larger dose was required than is recommended by that physician in order to have the desired effect."

He also gives a quotation from one of M. Trousseau's clinical lectures which will explain this physician's opinion and practice in this disorder:

"This infirmity I consider a neuralgia or *névrose*, and it is much more common than is generally believed. It has been thought nearly incurable; but, with belladonna, is one of the most easily cured of all diseases.

"*Those children who are troubled in the daytime are not cured by belladonna.*

"A child going to bed with an empty bladder will pass his water during the first two hours, sometimes during the first hour; placed in bed at nine o'clock, the bladder will throw off its contents at ten o'clock, and then retain them until eight o'clock the following morning, being full at that time. The accident generally happens once only during the night, but sometimes twice. During the first hours of sleep, you generally find in young men and children an erection. The question may be asked, whether something of the same kind may not take place in the bladder. This complaint is most common among young girls, and is generally cured spontaneously at the age of puberty; but when this is not the case, do what chance has shown to be efficacious.

"Two young girls afflicted with whooping-cough were treated with belladonna, and were cured both of the cough and of the affection now under consideration.

"*Treatment.*—The first precaution is to break up the bad habit of the organs. Wake the child at the end of an hour, and make him pass his water; after several days, make him at the end of an hour and a half. This is only accessory.

"Give at the moment of going to bed, ext. belladonna in pill, commencing with gr.  $\frac{1}{4}$ , waking the child as has been described. After eight days, increase the dose to double; at the end of eight days more,

treble it, waking him later and later, and finally not at all. When during fifteen days he has been free from the difficulty, diminish the dose, or give it only every second day, then every third day, &c. Even if the child does not regain its bad habits, renew the medicine after two, three, and sometimes five months' cure, for he may then again begin to wet the bed. Resume the medicine, as first given, several times, making the intervals longer and longer.

"The incontinence may be aggravated by eczema, caused by the constant irritation of the urine; and the urethra becoming inflamed, the desire to pass water is increased. For this state of things a sol. sulph. zinci is the best application.

"If belladonna fail, try strychnine, flagellation, and stinging with nettles. The two latter may be useful as means of intimidation, or perhaps they may have some reflex power."

#### ART. 141.—*Treatment of Albuminuria in Infants.*

By Professor MAUTHNER.

(*Journ. des Maladies des Enfants*; and *Edinburgh Monthly Journal*, July, 1855.)

The treatment of this affection, like all other complications of dropsy, has hitherto consisted in the administration of diuretics. In the dropsy succeeding scarlet fever, and which is accompanied with hematuria and albuminuria, Professor Mauthner, on the contrary, scrupulously avoids everything which, by leading to an increased excitement of the kidneys, may produce inflammation of these organs, or possibly give rise to Bright's disease. He has seen many cases where a cure has been obtained by well-regulated dietetic treatment, when diuretics have failed in checking the progress of the disease. In such cases he recommends the exclusive use of milk, and of rice and milk, and allows in addition a decoction of gum or of linseed. Under the influence of a milk diet there is established an abundant flow of non-albuminous urine, while the dropsy in consequence disappears. If the administration of milk is not followed by recovery, the effect of alkalis ought to be tried, with the view of modifying the urinary secretion. The medicine to which the preference is given is urea, either in its pure state or as nitrate. Taken in the dose of from one-third of a grain in powdered sugar, this medicine, when it reaches a dose of six or eight grains, determines an abundant secretion of urine, and the dropsy rapidly disappears.

#### ART. 142.—*Case of Spina-bifida cured by excision.*

By Dr. J. C. NOTT, of Mobile.

This case possesses novelty sufficient to deserve re-publication.

CASE.—The subject of this case, aged one month, was a male, and the child of an Irishwoman; it presented, about the middle lumbar vertebra, a tumour an inch and a half in diameter, nearly circular, and elevated about three-quarters of an inch. The appearance of the tumour was unusual, and the first impres-

sion on my mind was that of fungus hematodes; the summit was nearly flat, of a reddish-chocolate colour, and in the centre was a thin pellucid membrane of about three fourths of an inch in extent, through which could be seen serous fluid. The most graphic description I can give of the appearance of the tumour is that it resembled a half ripe carbuncle with a Malaga grape buried in the centre; had it not been for this deficiency of skin in the centre I should have been much embarrassed to form a diagnosis. Guided by a case somewhat similar, though smaller, reported by Dr. Mott, of New York, in his Appendix to Velpeau, I determined to extirpate the entire mass. I accordingly, on the 15th of March, 1855, in the presence of Drs. J. Hamilton, Vetchum, and Anderson, and my student, Mr. Childs, inclosed the tumour by two elliptical incisions in the direction of the spine, and dissected it out completely; the tumour was found to consist simply of skin, cellular tissue, and the membranes of the spine distended with serum. After the sac was removed, an opening into the spinal canal was exposed about the size of the end of the finger, and a tablespoonful of fluid escaped.

It was then dressed by bringing the edges together by a single pin and twisted suture, and placing above and below strips of adhesive plaster.

The dressing was removed on the third day, and complete adhesion had taken place by first intention, except the portion included between the pin and ligature, which sloughed; this left a narrow, gaping ulcer immediately over the opening in the spine, and I felt some apprehension about the result. I did not reflect on the extreme vascularity and tenderness of the skin of a child a month old, and put too much stress on a single point; it would have been more scientific to have made a longitudinal cut on each side to free the skin, and to have used two pins instead of one. The case, however, did well; granulations were thrown out, and the ulcer soon closed, and at the end of two weeks the healing was perfect, and the parts firm and solid.

The child had no constitutional disturbance whatever; slept and nursed as usual. Two months have now passed, and the cure seems to be complete.





# REPORTS

ON THE

PROGRESS OF THE MEDICAL SCIENCES.

*July—December, 1855.*

THE intention of the following Reports is to pass in review the principal additions to each department of Medical Science, which have been placed on record during the preceding six months. It is not contemplated that they should be confined exclusively to the notice of what is new; any fact or doctrine which may be considered practically useful, will, although not strictly novel, be regarded as worthy of commemoration. It must be obvious to all who are aware of the immense mass of information which is almost daily put forth by the medical press of this and other countries, that the notice of every subject would be an impossibility. It therefore devolves upon the writers of each Report, to select only such articles for retrospection as may possess superior recommendations, either of an intrinsic character, or in relation to the main end and aim of all medical knowledge—the alleviation of suffering and disease.



## I.

### REPORT ON PRACTICAL MEDICINE, &c.

*A Manual of the Practice of Medicine.* By GEORGE HILARO BARLOW, M.D., Physician to Guy's Hospital. (Churchill, 12mo, 1855; pp. 706.)

THIS volume appears before the world in very good company—in company, that is to say, with the excellent Manuals which have made Mr. Churchill's name so familiar to the medical profession. It also emanates from a quarter which must command respect. It has been long expected, and it will, no doubt, be eagerly and extensively read. And so it ought to be. At the same time we cannot refrain from expressing a regret that a work, which is so sure of a large audience, and this a young audience, should not represent a little more correctly what we conceive to be the “feelings” of the profession at the present time. It dwells, indeed, too much upon the old bugbear of inflammation, and says too little of the various forms of degeneration; and it is to be feared that the student who makes this manual his sole text-book may be in danger of thinking that the diseases of the present day are more active than they really are, and so fall into the mistake of adopting a line of treatment of unnecessary severity. We regret to have to make this remark: at the same time we think a great part of this deficiency may be corrected on a future occasion.

It is not necessary to give a systematic analysis of a book of this kind, and all that we have to do is, announce its appearance, and give an illustration or two of its contents. As to the plan, this is based upon the etiology, or, as the author terms it, the natural history of disease.

The volume opens at the chapter on fever, and here we will find our illustrations. We select the remarks upon the pulse, and the use of wine.

“Few of the phenomena of fever are more interesting or more instructive than the state of the pulse; and it is by the indications which it affords, that we are enabled, more than by any other class of symptoms, to regulate our prognosis and our treatment. One of the effects of the poison being upon the blood, the mutual affinity between that fluid and the tissues is weakened, and one of the moving powers of the circulation annulled or diminished, and consequently we find the heart labouring to overcome the obstructed circulation; but in this instance we have not,

in the ordinary fevers of this climate, the increased tonicity of the arteries which exists in inflammation, and consequently the pulse is sharp and full, but never, except in inflammatory fever, hard. As the fever continues, the powers of the system, and consequently the contractility of the heart, failing, we have the pulse weaker and weaker, and at the same time as soft as at first, or even more so, from the diminishing tonicity of the arterial coats. Owing to the continued efforts of the heart, and the persistent obstruction in the capillary circulation, there is not uncommonly a recoil to be felt, giving the sensation to the finger of a back stroke. Another effect of the continuation of the obstruction to the extreme circulation, conjoined with the continually diminishing power of the heart, is, that the latter being unable to empty itself, and therefore continually exposed to the presence of its natural stimulus, is incessantly excited to contraction, the effect of which is great frequency of the pulse, which is often commensurate with its debility. With this state of the heart there may be a tendency to the back stroke, and the result will be a kind of struggling or throbbing pulse, which is always a sign of imminent danger.

“When there is a subsidence of the fever, whether brought about or followed by anything like a critical discharge, or otherwise,—though it is to be observed that there is always a return of the secretions on its subsidence,—the healthy relations between the blood and the tissues gradually returning, the obstruction to the current is in some measure diminished; and therefore the pulse loses its sharpness, and the ventricles of the heart being better able to expel their blood, it becomes also slower: but the diminished contractility of the arteries continuing, with the generally exhausted state of the system, the pulse is soft and moderately full: and as the convalescence becomes established, and the secretions abundant, the quantity of the blood in the system being probably diminished, the left ventricle empties itself fully, but is slowly refilled. The force of its contraction no doubt remains somewhat weakened, but the same is the case with the contractility of the arteries, so that they are equally balanced, and the result is a slow, very distinct, and moderately soft and full pulse. It may be well here to remark that the conditions of pulse which have been here somewhat theoretically referred to the different periods of fever, are fully borne out by experience; a quick and feeble pulse being that always met with in the advanced stages of continued fever, the pulse of convalescence being distinct and slow (sometimes below the healthy standard) and moderately soft and full.

“Upon these grounds, but still more upon almost universal experience, the frequency of the pulse is of the greatest importance in fever; its not exceeding 100 is in general a favorable sign, when it exceeds 120 in adults the danger is great.”

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“Now as wine is a direct stimulant to the heart and large vessels, and has but little influence over the extreme circulation except through the heart, it is certainly highly desirable to delay its use until the tendency to capillary obstruction has passed away; or, in other words, until the fever itself has subsided, which will generally be shown by the pulse becoming slower and fuller, but at the same time soft and very compres-

sible. When this is the case, although the tongue may be brown, the patient almost unconscious, and in the most abject state of prostration, we may confidently expect the greatest benefit from wine freely administered. It is not always that the indications for its use are so clear, as in the worst cases we often find the central moving powers of the circulation to be sinking, whilst the febrile state and the consequent difficulty of the extreme circulation continue: under these perplexing circumstances, we must be guided by other conditions as well as by the pulse. Thus, in cases where wine is most required, the patient generally lies prostrate upon his back and sinking lower and lower in his bed, the countenance is sunken, the eyes hollow, the surface is inclined to cold at the extremities, though it may be hot about the trunk. If, however, this heat is attended with dampness we may with more confidence administer wine. The state of the pupils will often aid us, for in general, stimulants are better borne with a full than a contracted pupil. The tongue in such cases will generally be brown from a crust of sordes, which also covers the teeth and gums. The state of pulse which affords the most certain indication for the use of wine has just been described, but as in the worst forms of fever we must not wait for that which may never show itself, we must consider feebleness and compressibility as themselves indications for the use of wine, provided the other signs of prostration are likewise present.

“A rule has been proposed by Dr. Stokes which is certainly worth attending to, though it may not be at all times applicable; namely, that when the first sound of the heart is nearly lost and becomes much more feeble than the second, wine is indicated. It will not be safe in all cases to wait for this symptom, but certainly where it does occur, it is in general a sign that stimulants are required. As regards the quantity of wine to be given, it is impossible to lay down definite rules, as it must be given according to its effects rather than by measure. Where the signs of sinking are not very urgent we may begin with an ounce of sound port or sherry, which may be diluted with an equal quantity of water, and given about four times in the twenty-four hours, or even a less quantity may be given at shorter intervals. It will always be necessary to watch most carefully the effect of the wine, and if it cause increased heat of the head or active delirium, or if the tongue become drier under its use, or the pulse more frequent and sharper, without any increase of volume, it must be withdrawn: but, if the tongue become moister or the pulse less frequent or fuller, and especially if the patient should get some sleep or appear more tranquil, even though the depression become more alarming, its use must be continued and the quantity increased, and this must be done without limit as long as the prostration continues, or the pulse appears to become more feeble. Sometimes ten, twelve, or more ounces must be given in the day, and in cases of extreme prostration, brandy and also ether must be given in addition; but such are almost desperate, though certainly where there has been a slow and compressible pulse, patients have sometimes been saved by the timely administration of the strongest stimulants.”

We leave these passages to convey the favorable impression which they are well calculated to convey, merely adding that the whole book is eminently practical and plain, and that the most careless reader



can have no difficulty in learning its lessons and carrying out its instructions.

*Report on all the cases of Fever which occurred in Guy's Hospital during the year 1854, with Remarks having especial reference to the Typhus and Typhoid distinctions.* By SAMUEL WILKS, M.D. (Guy's Hospital Reports, 1855.)

The question which at present excites most interest in connexion with fever, is the identity or non-identity of typhus and typhoid fever. This question is, no doubt, set at rest in the minds of many. It is set at rest in our own mind, and we fully agree with Dr. Jenner in thinking that typhus and typhoid are different affections. But this is not the case with all, and therefore we are glad to have the additional evidence which is contained in the present report.

This evidence, then, consists of the notes of no less than 187 cases. It is clear and conclusive; and, after reading it, it is scarcely possible that any unprejudiced person should continue to believe in the identity of these two forms of fever. We cannot give the cases; we can only give some of the comments upon them.

"It will be seen," says Dr. Wilks, "that two very different rashes are described, and that these have occurred in various degrees of intensity, and that they have sometimes been altogether absent. In the latter cases, it will be observed, however, that with the exception of the eruption, the symptoms in every respect followed a particular type. Regarding, first of all, the cases which had the rashes, it will be seen that those in which the rose eruption existed had, without exception, diarrhoea as well as other symptoms of intestinal disease, and, in fact, they were all well-marked cases of typhoid fever. Those with a mulberry rash had for the most part a comparatively healthy state of bowels, although it is mentioned that diarrhoea occurred in one or two instances, but the importance of this symptom we shall presently discuss. As regards the relation of the degree of the virulence of the fever to the amount of the rash, there is not much to be learned, as it will be seen that the violence of the former, and the degree of the intensity of the latter, bore no exact proportion to each other; although it is true that the mildest cases had no rash, and that in the most severe it was generally present, and also that no fatal case in the above list occurred when it was altogether wanting. (I have seen typhoid fever fatal, however, without any eruption, although I have never witnessed this occurrence in typhus.) The rash argues, therefore, a certain amount of intensity of the fever, but is not proportionate to it. Its presence in all the fatal cases proves so much. Eliminating, however, the extreme instances, and regarding only the majority of the ordinary cases, we cannot say that the amount of the rash, and the severity of the disease, as shown by other symptoms, were proportionate. It will be seen that in some cases, as No. 73, where the mulberry rash was well developed, the symptoms were, comparatively speaking, mild, and the patient recovered without the administration of any stimulant. On the other hand, No. 90 is an example of a man who had no eruption, and yet who narrowly

escaped death. The same remarks hold good of the rose rash. I have more than once seen out-patients present themselves at the hospital with fever, in whom a rose rash existed, and the accompanying symptoms were of the mildest form. In case 144, if the eruption had not been present, the existence of fever would hardly have been determined; and the patient was so little ill, that his principal trouble was the order for keeping his bed. It is clear, then, that there are other causes besides the mere virulence of the disease, which determine the presence and amount of the eruption, and I am inclined to think that these depend upon accidental causes, as some peculiar idiosyncrasy of the patient, which predisposes him to affections of the skin, or upon some condition of the integument itself, which makes it more liable to assume a morbid state in one person than another—in the same way as some are more prone to have pulmonic, and others to have cerebral complication when attacked with fever. I would be understood to say, that the presence and peculiar character of the rash is primarily determined by the specific agency in operation, and that, to a certain extent, the more powerful that agency the greater is the tendency to the skin affection, as evidenced by its existence in fatal cases, but that after the peculiar rash has been so produced, the fact that it does not vary in intensity with that of the fever, proves that some other conditions are present to influence its development, and these, I cannot but think, are mostly accidental, and due to the nature of the skin itself, or some such secondary cause. In the analogous instance of scarlatina, we see the mildest and the worst cases where little or no eruption exists, so that no relation can be determined between the virulence of the disease and the amount of the rash.

“Now with reference to the two varieties of eruption,—the typhus and the typhoid,—there is, as a rule, a marked difference between them. The clear skin with the pink spots scattered over it in the latter, and the mottled skin of the former, are generally sufficiently distinct. How the two can be confounded, in the majority of instances, I cannot well imagine, and yet some writers at the present time have not perceived a difference. I think this must arise from a want of extensive observation, for no doubt occasionally, in particular instances, great difficulties do arise. Thus, sometimes we see patients exceedingly low, the whole surface of the skin dusky, perhaps also very dirty, and covered with flea-bites, and, in addition, numerous acne and other pimples; and amongst all this is to be observed a recent rash, which fades on pressure. In some such cases I have found it almost impossible to say whether these were the rose spots of typhoid, or the larger marks of the mulberry of typhus. Such instances have appeared to favour the opinion that no distinction existed between them; that the two rashes were mere modifications of one another; or, if distinct, that they could occur at the same time, and, consequently, that the two forms of fever were undistinguishable. If the rash alone were to determine the whole question of the generic differences of fever, such cases would no doubt often warrant such a conclusion; but the whole history of the case must be taken to discover its nature. For example, No. 127 was that of a woman, who, on entering the hospital, was too ill to give any account of herself. The whole body was covered with spots, ill defined, and the



skin was dusky, and it was difficult to say to which kind the eruption belonged. Thus she remained two days, when the cheeks became flushed, the bowels very loose, and a friend who now arrived stated the duration of the illness, and the previous existence of diarrhœa. At the same time the spots became more characteristic, and the case run a uniform one of typhoid. In this instance, with the absence of history, and no very marked symptoms, the eruption would altogether have failed to determine the nature of the case at the time of admission. In its subsequent course, however, as well as in its previous history, it showed itself to be one of ordinary typhoid, and, therefore, teaches us that the obscurity of the rash alone cannot warrant us in the assertion that typhus and typhoid are inextricably united. In two other instances, also, there was a difference of opinion among good observers as to which kind the rash was to be referred. These are, however, exceptional cases, for in the majority it must be affirmed that the two eruptions are well marked, are characteristic, and cannot be confounded. It has been farther questioned, even allowing the existence of a general distinction between them, whether they constitute more than varieties of each other, dependent upon secondary circumstances, and whether the two may not in some instances be combined. In such cases as I have just mentioned, it would be difficult to answer such a query positively or negatively; for amongst a thick typhoid rash it would be impossible to say that some spots did not remain during the whole time of the fever, and so far resemble the larger ones of the typhus eruption, or that in some cases of typhus rash a few rose spots were not present for a time, and then disappeared. There is no proof, however, that such cases, although surmised, do ever occur. The general fact still remains, that a well-marked rose rash belongs to fever having peculiar symptoms, and that an equally well-marked mulberry rash belongs to another variety of fever, having its symptoms, one of the most striking of which is (as the above list shows) the diarrhœa and diseased intestine in the former, and the perfectly healthy intestine, in those who were examined after death, in the latter. With regard to the duration of the eruption in each variety, it was found that the mulberry rash persisted throughout the disease, and that the rose rash constantly faded and reappeared in a succession of crops. I think Dr. Jenner mentioned four days as the duration of a single macule, and I believe the spots usually begin to disappear about this time, but I have found it not unusual for them to remain six or seven days. There are two other facts worthy of remembrance in the history of the rose rash: the one is, the occasional reappearance of the eruption during a relapse, and the other has reference to a difficulty which sometimes occurs in judging of the nature of the rash which is about to appear at the onset of the fever; for, previous to the development of the perfect form, a general pink efflorescence of the skin may exist, and be easily mistaken for the incipient stage of the mulberry rash. In conclusion, then, we must say, with respect to the eruptions, that two well marked varieties characterise two forms of fever, but that in some exceptional cases they are for a time with difficulty distinguishable."

Afterwards, Dr. Wilks speaks of that condition of the intestine which constitutes the main distinction between the fevers in question.



“We have seen that in all those cases where the rose rash existed, diarrhœa was also present, besides the other well-known symptoms of intestinal disease,—as the full doughy abdomen, the flushed cheek, the red tongue, &c. It will be seen, also, that diarrhœa is mentioned as having occurred in some cases where the mulberry rash existed; but it must also be observed, at the same time, that no concomitant symptoms of bowel affection were present, and that in the fatal cases, with this eruption, there never was the slightest morbid appearance found in the ileum. In perusing the cases of typhus where diarrhœa is mentioned, the observations which I made in my preliminary remarks must be remembered, that a loose evacuation is often to be anticipated at the termination of the fever, for I related cases where this had occurred, and no disease of the intestine had been found, and stated that, as a rule, on the post-mortem table, we see the contents of the bowels in typhus fever to be fluid. Whether the evacuation occurs as a crisis, I will not undertake to say; but I can state that it is by no means uncommon at the expiration of the fever, and when the mulberry rash is fading, for one or two large liquid fecal evacuations to take place. This symptom cannot, and ought not, then, to be constituted into a diarrhœa, much less to be taken as evidence of a diseased bowel. Notwithstanding, as I have myself witnessed, a difficulty has arisen with respect to this symptom when a patient has been seen during its occurrence for the first time. In two cases of typhus in my list, diarrhœa is said to exist at the commencement of the disease; but as both patients had taken aperient medicine before admission, the symptoms might justly be attributed to this fact. If, however, a little diarrhœa did take place occasionally in a case of typhus, it is no more than might be expected with such a condition of fluids as is supposed to exist in this disease, for its occurrence, we know, is very common in pyæmia and analogous affections, where the blood is in a dyscrasic state. In such diseases we do not look to this symptom alone as evidence of a diseased bowel, unless it be constant, and other reasons for suspecting its existence be present; but rather we consider it as one of the means which nature employs to carry off the poisonous matter from the blood. We must say, then, that a patient with typhus having occasionally a loose evacuation, presents a symptom which might be expected, and cannot be taken as any evidence of a diseased bowel. As a rule, however, it seldom occurs during the progress of fever, but not unfrequently at the close; and in most cases after death the intestinal contents are found fluid. In the 64th case, of a man who had typhus, and no diarrhœa, the intestine was found full of liquid fecal matter; and therefore, if this man had lived but a short time longer, he must have had a large liquid evacuation. Some years ago I witnessed the following case:—A woman, æt. 48, was very ill with typhus fever, and covered with a mulberry rash, of which some of the spots at the end of a fortnight had become petechial. A castor-oil injection was then administered, a copious fluid evacuation followed and continued until death, a few hours afterwards. This was apparently a case of typhus, dying of diarrhœa. The intestine, however, was found quite healthy. How far the treatment produced an over-action of the bowel, which increased the exhaustion and assisted in killing the patient, is questionable.”

The cases related in this report also show very clearly the very marked difference which there is in the natural history of these two fevers; and this difference is well put by Dr. Wilks, together with some practical bearings of the difference, which we cannot refrain from quoting.

“If, as we shall find, these forms of disease pursue a certain definite course of their own, it is evident it must be of the very highest importance to have an accurate knowledge of their duration, and, therefore, in any individual case about which we may be consulted, it will be our first duty to ascertain the exact date of the illness; for not only will this knowledge be necessary to form a prognosis, but upon it will depend in great measure the success of the treatment. In looking at the cases of typhus, it will be seen that on the third or fourth day of the disease, the patient is exceedingly ill; his nervous and muscular powers are quite prostrated; he is, perhaps, delirious; and at that early period his skin is covered with a rash. Now, in cases of typhoid, the early progress of the case is much slower. The patient, whom we may suppose to be taken ill at the same time as the typhus one, is still at his employment, while the latter is ill in bed, and does not seek admission until about the tenth day. He is then becoming very ill, and a rash is appearing. If he present himself earlier, as at the end of seven days, he walks to the hospital, and no appearance of eruption has yet been discovered; he still preserves his intelligence, and is able to give a complete history of his symptoms. These, at the onset, are insidious, as they are often at first not more urgent than those which attach themselves to a common cold,—a shivering, loss of appetite, slight headache, &c.,—sometimes an epistaxis. In typhus, on the other hand, the symptoms are at an early period of a much more violent character, the headache is often intense, and the depression very great, with an extreme aching of the limbs. The same rapidity of symptoms characterises typhus throughout its subsequent career: the subject of it being exceedingly ill on the fourth day, sinks lower and lower until between the twelfth and fourteenth day, when he begins to improve. The time of duration of the disease in all probability is uniform, but the want of exactness met with in this respect is due to the difficulty of ascertaining accurately the first onset of the symptoms. As a rule, the fever comes to an end on the thirteenth or fourteenth day. The typhoid, on the other hand, runs a much longer course. The patient with this disease is ill at least three weeks before any sign of recovery takes place, &c. The time of this occurrence is not so definite as in typhus, and which want of precision is due to the same cause—the difficulty of ascertaining the particular day, on which the symptoms commenced. It is never until the expiration of three weeks, however, that a change is observed, and this is generally seen to happen on some day during the fourth week. As I before said, I was hardly prepared for this uniformity in the course of the disease; but it affords an explanation of a fact which is constantly heard spoken of in reference to fever—the rapid progress of a case under one treatment, and the lingering nature of the illness under another. It will be seen by an observation of the above cases, that whether the patients entered the hospital early or late, the disease ran a certain course. Those which came in towards the close of the fever soon got better, and those which



came in early, daily got worse, and this was seen to occur under all kinds of treatment. Some simply had salines administered, some had tonics, and others stimulants from the beginning, but it was all the same, nothing staid the progress of the disease. I exclude in this statement the plan of Dr. Dundas, by large doses of quinine, of which I have little or no experience; and of course, in speaking of the duration of fever, I do not refer to the subsequent effects of the disease—as pulmonary or abdominal phthisis, &c.—which may delay the ulterior recovery to an indefinite period.”

This report does not throw much light upon one point,—and that is, as to whether typhus and typhoid are ever intercommunicable; nor was this to be expected in hospital practice. There was not a tittle of evidence, however, in favour of intercommunicability. The post-mortem disclosures present no novelty. They agree fully with those already known.

In a word, the evidence contained in the report is irresistible; and Dr. Wilks is quite warranted in saying, as he does say,—

“I say, then, as regards the typhus and typhoid genera, that in the majority of instances the two are plainly distinguishable, and that in the few cases in which some doubt may arise, on account of the obscurity of a rash or some other symptom, a light will subsequently be thrown upon them by the completion of the history, and it will then be clearly seen to which type they belong; and if there be yet one or two cases (in any given period) concerning which an obscurity hangs throughout the whole duration of the illness, they will probably be fewer in number than those instances in which an equal doubt may exist as to the presence of fever or a local inflammatory diseases of the head or chest. From such exceptional cases, therefore, it were no more just to confound the two species, than to say fever, pneumonia, and arachnitis were identical, because in some few instances it were not possible to distinguish these latter apart. I believe, then, myself, that the connexion between the two kinds of fever is no greater than exists between many other forms of disease; but the greater tendency to confound them has been owing to the want of a correct knowledge of their history, as well as to the non-recognition of some particular and important symptoms attending them.”

*Hypnotism in Hysterical Paralysis.* By JAMES BRAID, Esq., of Manchester. (‘Association Medical Journal,’ Sept. 14, 1855.)

We would wish to direct especial attention to the facts and arguments contained in this article, for, as we take it, the arguments are as sound as the facts remarkable. What *hypnotism* is, or wherein it differs from mesmerism, we shall leave Mr. Braid to tell us in his own words, for we can add nothing to the clearness of his description.

“It is no doubt known to many,” he says, “that in 1841 I entered upon the experimental investigation of the phenomena of Mesmerism. I did so as a decided sceptic in every respect, resolved, if possible, to discover and expose the trick by which certain phenomena then being exhibited in Manchester, were accomplished. I very soon discovered,



however, that there was a certain amount of truth, mixed up with what I believed to be error ; and I therefore resolved to attempt to separate the former from the latter.

“I was soon enabled to demonstrate that, by certain processes, some individuals were able, by their own unaided efforts, to throw themselves into an analogous state with those who were subjected to the Mesmerising processes. The most speedy and certain mode for accomplishing this was causing the subject to maintain a steady fixed gaze at any inanimate object, placed a little above the forehead, but so as to be seen distinctly by both eyes ; the subject at the same time concentrating his undivided attention upon said unexciting act. This was at once a most important step, as it proved the influence to be *subjective*, or a personal influence existing within the patient's own body, and not depending upon any influence *ab extra*, proceeding from the body of another human being. This inference was still farther supported by the fact that the varied nature and quality of the object gazed at seemed in no way materially to alter the subsequent results ; whilst, in highly impressible subjects, it was proved that the results depended so much upon the expectant idea in the mind of the subject, that any physical combination of circumstances whatever to which they were told to direct their attention, with the assurance that they would be thereby sent to sleep, was followed by sleep ; whilst the next moment they might comply with the same directions without going to sleep at all, if persuaded by the suggestion of a second party, or by a pre-existing idea in his mind to the effect that now the agency was to be inoperative, from some supposed change in the existing circumstances. It was moreover ascertained that, with some very susceptible subjects, the mere idea and belief that some particular process was going on at a distance, to send them to sleep, was sufficient to produce such result, even when no such process was taking place : and farther, in reference to the alleged power of the will of the operator to affect subjects, either near at hand or at a distance, after having carefully investigated the subject, I am warranted to state, as the result of my experience, that I have never found any influence whatever to be exerted over the patient by my *silent willing* ; but they seemed very quick to catch suggestions from the manners, looks, tones of voice, or physical manipulations of the operator ; and to become affected according to the purport of what they *inferred* to be the *will* and *intention* of the operator, and that even when he might be *willing* the *very reverse*. In short, my experience went to prove that the real efficient agency of all the different processes was merely as aids to assist the patient to induce in himself a state of mental abstraction or fixity of attention, in which the powers of his mind should be so absorbed by a fixed idea or given train of thought as, for the nonce, to render him dead or indifferent to all other considerations and influences which did not harmonise with the dominant idea in his (the patient's) mind at the time.

“As a very strong corroboration of the correctness of this view of the subject, I may state the fact that, from the difficulty of fixing the attention of idiots, all my attempts to hypnotise them have been unsuccessful, notwithstanding I have made many persevering efforts to do so.

“Again, in my experimental inquiry into *The Power of the Mind over the Body*, which was published in 1846, as the result of a laborious set of experiments, I was enabled to demonstrate that a sustained act of attention, directed to any part of the body, was followed in a few minutes by a change or modification in the physical function of the organ or part so regarded; the general result being an exaltation of function; but, curiously enough, with many individuals the very reverse result might ensue, from a dominant idea to that effect having existed in the mind of the patient previously, or from its being strongly imprinted on his mind at the moment by an audible suggestion from another person, in whose prediction he could repose confidence. The more vivid the imagination and fixed the attention of the subject to the expected result, the more certainly and vividly were the phenomena realised; and, after the processes for inducing what I call the hypnotic state, it was found that these physical phenomena could be produced, through the mental influences of the subjects, with far more certainty, celerity, and intensity, than in the ordinary waking condition. I therefore adopted the term hypnotism, or nervous sleep, to designate this peculiar condition of the nervous system, into which it could be thrown by artificial contrivance, and which differed in several respects from common sleep, as well as from the ordinary waking condition. In fact, hypnotism comprises, not one state, but rather a series of stages or conditions, varying in every conceivable degree, from the slightest reverie, with high exaltation of the function called into action, on the one hand, to intense nervous coma, with entire abolition of consciousness and voluntary power, on the other; whilst, from the latter condition, by very simple but appropriate means, the patient is capable of being speedily partially restored, or entirely roused to the waking condition. By this means, I maintain that the operator does not communicate any surcharge of a magnetic, odyllic, electric, or vital fluid or force, from his own body to that of the patient, as the real and efficient cause of the phenomena which follow, in altering or modifying physical action, or curing disease; but I hold that he acts merely as the engineer, by various modes, exciting, controlling, and directing the *vital forces within the patient's own body*, according to the laws which regulate the reciprocal action of mind and matter upon each other in the present state of our existence.”

Mr. Braid enters more fully into this part of his subject, and suggests several new terms, but we have said enough to make the cases intelligible which we are about to relate, and this is our sole purpose at present. These cases comprise four of hysterical spasm or paralysis (of which we take two), and one of impaired vision, also no doubt of an hysterical character. These cases we leave to tell their own story, merely premising that Mr. Braid does not always depend upon hypnotism, to the exclusion of the ordinary modes of treatment.

CASE 17.—In the spring of 1842, a girl was brought to me, under the following circumstances. She had been suddenly seized with violent tonic spasm of the hand and arm, so that her hand was rigidly clinched, the wrist flexed on the arm, and the arm flexed on the humerus, attended with considerable pain. Most of the servants, male as well as female, in a large hotel,



had exerted their utmost strength without being able to open the hand or extend the arm. A highly respectable surgeon was now sent for, who prescribed medicine internally, and the application of a large blister on the nape of the neck. There being an aggravation of symptoms, I was consulted, when I immediately recognised this as a hysteric case, for which I hypnotised her, and then, by gently titillating the skin along the course of the extensor muscles, they were immediately called into action, and the morbid action of the flexors at the same time withdrawn, by which simple means, by *art*, and without the slightest effort, I was enabled, in a few minutes, to effect what had resisted the strongest efforts of powerful men to accomplish; for the hand was opened, and the wrist and arm extended, and the patient cured in a few minutes; and she never had any subsequent attack of the sort.

CASE 19.—On the 11th of August, 1853, a young lady, 23 years of age, was sent to me from Berwick-upon-Tweed, by Dr. Johnston, of that city. The following is the history of this very interesting case. Four years previously, she had been seized with a paralytic dragging of the left leg, which became worse and worse, notwithstanding the most assiduous efforts of Dr. Johnston, who is a most experienced and scientific physician, aided by the opinion and advice of Sir B. Brodie, who had been corresponded with on the case by Dr. Johnston. Four months having elapsed without improvement, she was taken to Edinburgh for a consultation with Professor Syme, who examined her spine with great care, said he found no disease there, and hoped she would recover ultimately, also several years might elapse before such event took place; and that he could only, in the meantime, recommend attention to her general health, with exercise in the open air, by riding on a donkey. This was persevered in for ten months more, without the slightest improvement, when the patient was taken to London, to have the benefit of the personal examination and advice of Sir B. Brodie. Still no improvement ensued for months, and at length all treatment was abandoned. After this patient had been in this condition for twenty months, and after all treatment had been abandoned as quite inefficacious in her case, she at length gradually recovered the use of her limbs. From that period, she had occasional threatenings of a return of her old complaint; and, in the summer of 1852, she frequently felt as if her legs were being galvanized. In February 1853, she had a return of her paralytic affection, which continued for a fortnight; and, at the end of April 1853, she had another seizure, which had obstinately resisted all the best directed efforts of her old and tried friend, Dr. Johnston, for four months, when he sent her to me, to try the effect of hypnotism; which he had been induced to do from having read some remarks on the subject contained in my paper on “Hypnotic Therapeutics,” published in the July number of the *Monthly Journal of Medical Science*, for 1853. The following is the passage which impressed Dr. Johnson with the belief that hypnotism would be the remedy for his patient; and he immediately recommended it to the parents, and wrote to me accordingly, and sent his patient hither.

“The most striking cases of all, however, for illustrating the value of the hypnotic mode of treatment, are cases of hysteric paralysis, in which, without organic lesion, the patient may have remained for a considerable length of time perfectly powerless of a part or of the whole of the body, from a dominant idea which has paralysed or misdirected his volition. In such cases, by altering the circulation, and breaking down the previous idea, and substituting a salutary idea of vigour and self-confidence in its place (which can be done by audible suggestions, addressed to the patient in a confident tone of voice, as to what *must* and *shall be* realised by the processes he has been subjected to), on being aroused, in a few minutes thereafter, with such dominant idea in



their minds, to the astonishment of themselves as well as of others, the patients are found to have acquired vigour and voluntary power over their hitherto paralysed limbs, as if by a magical spell or witchcraft. Assuredly such cures are as important as they are interesting and surprising, because such cases may resist ordinary modes of treatment for paralysis for an indefinite length of time; but still the *rationale* is simple enough, when viewed according to the principles which I have already explained, of the influence of an expectant dominant idea, *either exciting or depressing natural function, according to the faith and confidence of the patient.*"

On the 11th of August, the above patient called on me, accompanied by her mother. The patient was a tall, handsome, and intelligent young lady, twenty-three years of age, five feet eight inches high, with figure well proportioned to her stature, so that when seated she had all the appearance of youth and vigour. When she attempted to walk, however, her paralytic condition of the left leg was very obvious, as she could only drag it along the floor at each step as far as the heel of the other foot, with the toes of the affected limb turned outwards. I had no difficulty in recognising the nature of the case; so that I at once assured both mother and daughter that I would make very short work of that case. The mother said she would be glad if it turned out so; but she uttered this in a tone of voice which indicated that she was by no means equally sanguine on the point as I was. Having seated the patient in an easy chair, I hypnotised her, and extended her limbs, and acted on them so as to change the previously existing state of the muscles. In about ten minutes I aroused her, and requested her to walk across the room, which she was enabled to do, lifting the left foot from the floor, and carrying it forward before the other in the usual way; which she had not been able to do for months previously. The improvement, although she was still a little lame, seemed greatly to surprise both mother and daughter; more especially, from the apparent simplicity of the means by which it had been accomplished. Next morning I repeated the operation, with still further improvement; and on the evening of the same day I operated again, after which she could walk up and down and around the room without the slightest appearance of lameness; and after a fourth operation, next morning, she could walk with the grace of a queen, or the agility of a sylph. Immediately after this operation, she rode to town, and there walked about through various shops and streets, as if she had never been lame at all. As the muscles of the other leg had also been somewhat affected, I recommended the patient to remain under treatment about a month, the more effectually to consolidate the cure; after which she returned home quite strong and active, and she remained so for twelve months.

During the summer of 1854, this patient had been so vigorous as to be able to climb the hills in the Highlands of Scotland as actively as her companions; but, during the autumn, from fatigue with a round of company, and anxiety about the health of a friend, her general health broke down; and, at the end of September, she had another paralytic seizure. As it had persisted for three weeks, she was brought to me once more; the mother of the patient expressing her fears, however, that I would not find hypnotism so successful as before, as her general health was so broken down on the present occasion. They arrived in Manchester at eight o'clock in the evening; and, as soon as they had had some refreshment, I told them that I intended to make the patient walk without lameness *before she went to bed*. The mother of the patient was quite incredulous: but I hypnotised, and acted as on the former occasion; and, in a quarter of an hour, the paralysis was quite gone, the patient walking without the slightest degree of lameness. After being

hypnotised again next morning, she felt as vigorous as before the attack; and all the constitutional ailments her mother had been so anxious about speedily disappeared also under hypnotism, and the patient has kept quite well ever since.

CASE 21.—On the 19th of June, 1854, I was consulted on the case of Miss R. Twelve months previously, she had had an attack of ophthalmia, which yielded to treatment so far that she was able to go out of doors in a month. She now had the misfortune to sustain a blow, from a pole falling upon the upper and left side of her head. Two or three days subsequent to this accident, she suffered severe pain from the blow, when suddenly she became quite blind of the eye on the same side, with dilatation of the pupil. For this affection her medical attendant again subjected her to a course of treatment; and the result was, that in four months sight was partially restored to this eye. At the beginning of January 1854, whilst reading the newspaper, this patient suddenly lost sight entirely of the other eye, with dilatation of its pupil, as had been the case previously with the other eye. Another surgeon was now consulted in the case; and a few days thereafter, whilst rising from the stooping posture near the fireplace, the patient had the misfortune to strike the same part of the head against the mantel-shelf which had sustained the former injury from the falling of the pole, which blow against the mantel-shelf was immediately followed by total loss of sight of the corresponding eye; and thus she required to be led about in a state of total blindness in both eyes. After treating the case for some time himself, the gentleman now in attendance, from a consideration of the obstinacy and importance of the case, recommended her to go to Dublin and place herself under the care of Mr. Wilde, a celebrated oculist in that city. This was complied with, and she remained under the care of Mr. Wilde for six weeks, during which period she went through a course of very active treatment, with decided improvement; for the iris had become somewhat irritable on the application of light, and she was able to discern large objects, but could neither see to read nor write. She now returned home, where the same line of treatment was persevered with, under the supervision of the surgeon who sent her to Mr. Wilde. After she had been at home for some time, and finding the improvement had become stationary, this gentleman recommended her to try hypnotism, and furnished her with a letter of introduction to me, detailing the history of her case. On examination, I found no apparent physical imperfection to account for the impaired vision; nor was there any pain about the head or eyes. The eyes had very much the appearance of an incipient case of amaurosis, only the pupils were not quite so much dilated. I suspected that the cause of the impaired vision was a want of sufficient nervous irritability in the retina, and, if so, that hypnotism would very soon relieve her. My first object was to apply a test by which I might be enabled to ascertain what amount of benefit had resulted from my processes. On presenting the title-page of a book to her, with the largest and boldest letters in my room, I found she could not discern a single letter, notwithstanding there were some letters a quarter of an inch long, and very bold open print. Having hypnotised the patient and directed the nervous power to the eyes, by wafting over them, and gently touching them occasionally, so as to keep up a sustained act of attention of the patient's mind to her eyes and the function of vision, she was aroused in about ten minutes. I now presented before her the title-page of the same book, when she instantly exclaimed, with delight and surprise, "I see the word commerce!" pointing to it. I told her she would see more than that presently; and in a little while she exclaimed, "I see commercial," then "I see dictionary;" and shortly after, "I see



McCulloch," the name of the author; but she could see nothing more. I told her that after a little rest, I felt assured she would see still smaller print and, after a few minutes, she was able to read "London, Longman, Green, and Longmans." Such was the result of my first process. After a second hypnotic operation, next day, the patient could read, when first aroused, the whole of the title page of a pamphlet; and, in about five minutes after, she read two lines of the text. After another operation, the same day, she could read the small close print in the appendix; and was able the same evening, to write a letter home reporting progress, for the first time for twelve months. She only required two more hypnotic operations, when she was found able to read the smallest sized print in a newspaper; after which she left me, quite cured, and, as I have heard, she has continued well ever since.

*Considerations respecting Paralysis of the Sympathetic.* By J. HANDFIELD JONES, M.B., F.R.S. ('Lancet,' 21st and 28th July, 1855.)

The principal object of the writer of these papers, and that with which we are here concerned, is to apply some recent experiments of M. Claude Bernard upon the sympathetic to the interpretation of certain obscure forms of vascular congestion. These experiments, in the author's opinion, suggest the idea that these forms of congestion may be the consequence of paralysis of the sympathetic; and this the more, because this symptom in many cases has been found to subside under the administration of nervine tonics. In this opinion we are quite disposed to agree. Indeed, we think that Dr. Jones has very happily brought the light of true physiology to the elucidation of a very important and very obscure point in pathology.

It is not necessary to recapitulate the results of M. Bernard's experiments—for these are well known—but it may be well to mention Dr. Jones's individual experience in the matter. On repeating these experiments on a cat, then, the results arrived at agreed almost to the letter with the result arrived at by the French physiologist. "The temperature of the left ear (the side operated on) immediately after the operation was 93° Fahr.; of the right 91.5°. The next morning the sound ear was 86°; the left, 97.5°. There was a mucous secretion from the inner canthus of the left eye, but the conjunctiva was not much injected; the left pupil was contracted much more than the other. On the right side, close to the axilla at the root of the neck, the temperature was 97°; on the left at the corresponding spot, 96°. For the next four or five days there was considerable conjunctivitis, with copious muco-purulent discharge, but after this date the conjunctival inflammation and mucous secretion diminished considerably. At the present date—*i. e.*, sixteen days after the operation—the left ear is 15° higher than the right; there are red vessels in it distinct to the naked eye; the conjunctivitis has almost entirely subsided, but the fold of mucous membrane at the inner canthus is very prominent. The pupil is much contracted. She takes food well now, which was not the case for the first five or six days after the operation. I should mention that before dividing the nerves in the neck I endeavoured to divide those going to the left kidney, but failed to do this satisfactorily.



The shock and depression of a severe double operation would naturally induce a state of debility, which, as Bernard states, promotes the occurrence of conjunctival inflammation. In my experiment the hyperæmia of the ear has certainly been a very constant and noticeable phenomenon; it has not as yet appeared to decline at all. It is a very interesting fact, stated by Bernard, and conformable to what might have been anticipated, that galvanizing the upper end of the divided sympathetic causes the disappearance of the phenomena produced by its division, and not only so, but actually the development of the converse state. Thus the contracted pupil becomes wider than natural, the depressed eye projects, the hyperæmic parts become pale, and the temperature sinks below the normal figure. What would not have been expected, is, that the ear of the sound side, during the application of the galvanic stimulus, rises in temperature almost as much as the other had done after the division of the sympathetic. The same may be said of the results observed during the administration of chloroform; when anæsthesia was complete, the ear of the operated side became cold and pale, while the other became more hot and injected. This alternation of phenomena seems to indicate something like an association in action of the two sympathetic cords, as if the throwing of one into an unusual state caused an opposite state of the other. It seems natural to suppose that the elevation of the temperature, coinciding as it does with hyperæmia of the part, stands to the latter in the relation of effect to cause; that, in fact, the part is hotter because it contains more blood. Bernard, however, opposes this view, because the temperature does not vary when the hyperæmia declines, because hyperæmia occurs when the fifth nerve is divided, but is attended with a diminished temperature, because ligature of the veins of each ear, and consequent gorging of the vessels with blood, lowers the temperature of the parts, which again rises on the side on which the sympathetic is subsequently divided. It is true that if the carotid is tied, and the sympathetic afterwards divided on the same side, no calorification takes place; but if the sympathetic be first divided, and calorification have come on, ligature of the carotid does not lower the temperature to that of the sound side. Much yet remains to be made out before a full explanation can be given of the various phenomena just mentioned, even supposing that they are all found to be of constant occurrence; but for the present it may be stated that section or destruction of the sympathetic filaments proceeding to a part causes increase of heat which is constant, and hyperæmic injection more or less considerable, not always persistent, but capable of arriving at the condition of severe inflammation in states of debility or depression of the general system. That the hyperæmia is the result of dilatation of the arteries, in consequence of paralysis of their contractile coats, is not so certain, though it seems to me the most probable opinion. Bernard states that the first effect of the division is contraction of the carotid, and he looks upon the subsequent distension of the vessels as the result of afflux of blood to the part. However, the contraction of the pupil, which is doubtless occasioned by the paralysis of the radiating fibres of the iris, seems to indicate that the loss of the influence of the sympathetic does tend to

paralyse contractile tissues, and the result above mentioned, of galvanizing the divided nerve, supports the same idea. Mr. Wharton Jones relates an observation in his paper, contained in the thirty-sixth volume of the 'Medico-Chirurgical Transactions,' which is further corroborative. He finds that after removal of the lower part of the spinal cord and the roots of the nerves, the arteries of the webs retain all their contractility, or are even more than usually disposed to be constricted. If now the ischiatic nerve be divided on one side, 'the result is that the skin of the extremity subjected to the experiment becomes, even to the naked eye, redder from vascular congestion than that of the opposite extremity, and on examination of the web under the microscope the arteries are found considerably dilated. In the web of the opposite extremity, on the contrary, the arteries are seen still much constricted, some even to closure.' "

The class of pathological facts upon which these experiments have a direct bearing is illustrated by an interesting case from Dr. Graves' 'Clinical Medicine,' (first edition, p. 868,) and this case is made the text of the memoir. In it the prominent symptoms were severe paroxysmal attacks of pain, heat, and vascular congestion of both feet and legs as high as the calf. The attacks generally commenced at night, with heat and tingling of the sole of the foot; as it proceeded, the parts affected became more and more hyperæmic, till at length they were swollen, smooth, and shining, and almost the colour of a ripe black cherry. "When the hot fit ceases, the slight swelling and this discoloration subside, and the affected parts remain during the next stage, pale, deadly cold, and comparatively free from pain. While one leg is in the hot stage, the opposite leg is cold and pale, but free from pain; but as soon as the pain and heat have disappeared in the limb first affected, the same series of phenomena commence in the other leg, and last for the same length of time; after which both limbs are in their natural state, and for two or three hours she is comparatively free from suffering, although some uneasiness still remains, which she compares to a numbness, or some such morbid sensation not easily defined." The paroxysms occurred daily, but the period and the duration of them varied, the intermission, which had been only three hours, being prolonged to eight at a later date. The affection came on after an attack of diarrhœa, which caused much debility; but she had previously been in a bad state of health, and the catamenia were suppressed. Improvement in health, and restoration of the natural secretion did not, however, induce any alleviation of the disorder. No treatment was of any avail, though all possible measures were adopted. No paralysis, or impairment of motion or sensation, or alteration of structure, took place in six years, during which time the patient was under observation. This case, which was a source of perplexity to Dr. Graves, would certainly seem to meet with its explanation in the experiments under consideration.

Dr. II. Jones, however, does not content himself with this case. On the contrary, he adduces cases out of his own experience of greater or less significance. Thus :

CASE.—Dr. Vernon has mentioned a case to me which fell under his



notice, where, during attacks of pain of neuralgic character, the eye and cheek became the seat of extravasation. The patient was affected with syphilis. The pain would last very severely for forty-eight hours, then begin to remit, and at the same time a blush would appear in the part, which increased up to actual extravasation. Black and blue discoloration subsequently occurred. In this case, which has some resemblance to Dr. Graves's, there was hyperæmia of such intensity as to cause actual hæmorrhage. Minor degrees of hyperæmia are common in frontal neuralgia; the eye becomes red from injection of conjunctival vessels. Hyperæmia sometimes occurs in one part as the result of altered innervation of another at a distance. This is exemplified in the flushing of the face which so often occurs in persons of weak digestive power after meals. The following instance of this state is interesting, from its association with other indications of disordered nervous action:

CASE.—Mrs. C.—, of rather large, lax habit, about mid-age, mother of five children, daughter of a lady who has had repeated attacks of severe neuralgia, had chorea herself at the age of fourteen. She has much exertion with domestic cares, and her energies are overtasked; she feels that she could sleep much longer than she allows herself to do. Lately her face has begun to twitch, the mouth being frequently drawn awry, so much so that she is observed sometimes to hold it with her hand to steady it. Immediately on eating, her face and neck become quite suffused with blood—crimson—so that she does not venture to dine out. She is wonderfully better in frosty weather. At times she has sensations as if the top of her head were held by tense cords. In this case, there is evidence of disorder of the whole nervous system, the cerebral centre, the sensory, motor, and sympathetic nerves being all affected. The pathological state is one of defective nervous power, with undue excitability, and the tonic influence of cold weather seems to be the most effective remedy.

CASE.—M. A. B.—, æt. 19, housemaid, was admitted on March 19th. Is much confined to the house, and goes out only once a week. The bowels regular; appetite good; digestion good; pulse excited; skin warm; tongue a little white, with some red papillæ. Has felt faint and weakly lately. Head hot and aching; sleeps well. Her aspect is now sanguine; but she is generally pale. Ill five days. Her cheeks are flushed, burning, and swollen, and of a decidedly redder colour than natural. The redness of face has increased since its coming on, which was sudden, first observed on rising one morning. I gave her iron with citrate of quinine, eight grains; tincture of hyoscyamus, fifteen minims; peppermint water, one ounce three times a day.

March 26th.—Cheeks not near so hot; feels much better.

April 16th.—Is reported quite well.

CASE.—E. Q.—, æt. 33, female, cook, single, resident in the same house as the preceding patient; admitted on March 19th. Is tall, slight, with rather patchy red face; slight eczema of right cheek. Ill six months. Suffers with redness, and burning sensation of cheeks and all her face, increased by excitement and by heat of fire, or drinking beer, not by lying down. At the same time, her head feels as if it were bursting with the throbbing. Sleeps well; catamenia regular; bowels regular; tongue clean. Takes food well. Pulse full and excited. I marked the case as of the same character as her fellow servant's who came in just before her; but noticing that there was more appearance of actual inflammation in the latter than in the former, I modified the treatment, giving her disulphate of quinine, three grains; dilute sulphuric acid, ten minims; compound infusion of gentian, one ounce, three times a day; and desiring her to take daily open air exercise, which she had very seldom had. After a week the pulse was less full and excited (thus evidencing



more tone); the face was not materially altered, but did not appear so red. I added sulphate of magnesia, fifteen grains, to the draught, and omitted one grain of quinine. The next week the face was less flushed. She had much headache. I gave her then, besides the mixture, an ointment to apply to the face, composed of zinc ointment and acetate of cerate of lead, of each two drachms. She continued this treatment for three or four weeks, still following her occupation, but not in the least my prescription of out-door exercise; and the result was that her face quite recovered, but she continued to suffer much with her head. After this she took citrate of iron and quinine, ten grains, three times a day, for another fortnight, at the end of which she presented herself quite recovered. The face was decidedly less injected than I had yet seen it; but the skin showed some degree of roughness.

These two cases are of a very ordinary, commonplace kind, so much so that I almost feel I ought to apologize for quoting them, and yet, though miniatures, they may faithfully portray graver affections. In the first there was considerable hyperæmia, occurring in a weakly, house-confined girl, without any special disorder of health or cause to account for it. Ninety-nine such cases out of a hundred would have complained of pain in the left side; but in her the nerve disorder showed itself in the form of a semi-paralysis of the vascular nerves of the face. The tonic which cures most cases of ordinary neuralgia cured her. In the second case there was a very similar but more inflammatory condition of the face, with a special cause of aggravation, (her employment,) and at the same time there was evidence that the brain was affected in a like way. One cannot but think it very possible that the hyperæmia, which on the face showed a proneness to become inflammation, might do the same in the brain, and, if long-continued, cause thickening of the arachnoid, or some degree of damage to the delicate grey neurine of the convolutions. It is specially interesting to find that the *visible* and the *concealed* hyperæmia disappeared alike under treatment, which was essentially tonic.

Dr. H. Jones also enters at some length into certain considerations respecting the relations which exist between neuralgia, ague, and some asthenic congestions, and between them all and paralysis of the sympathetic, but our space obliges us to limit ourselves to the point to which we have directed attention, and which is evidently of considerable practical importance.

*On the Employment of Injections into the Bronchial Tubes, and into Tubercular Cavities of the Lungs.* By HORACE GREEN, M.D. ('Transactions of the State Medical Society of the State of New York,' 1855, and 'American Monthly Journal of Medicine,' January and July, 1855.)

On more than one occasion we have called attention to the plan of applying caustic solutions to the interior of the larynx and trachea, as carried out by Dr. Horace Green. The possibility of carrying out this practice (which was at first violently contested) has, we think, been sufficiently demonstrated, as well as the advantages which may result from it in some cases; but there are still sceptics to be found, and for their sake Dr. Green has been led to seek for further proofs, and these proofs have led to the institution of the mode of treatment

about which we have to speak presently. The further proof consisted in the introduction of an appropriate tube into the windpipe, and in making the patient breathe through it. The experiment was variously modified. The patient was made to blow out a candle by the draught through the tube. A small bladder was fastened to the extremity, and this was seen to collapse or expand as the chest expanded or fell. The inward and outward currents through the tube were shown by the movements of a feather held at its opening. And so on. These experiments were tried on many subjects, and to the satisfaction of many credible witnesses whose names are given; and after them, there can be no rational doubt as to the possibility, with proper precaution, of passing an instrument fairly into the air passages. But this is not all. On the contrary, Dr. Green was carried away by his imagination far beyond this point. He had a tube in the air passages, and what was to prevent him from passing injections through it into the lungs? Why not even inject a vomica with fluids which should favour the process of healing? The idea was no sooner conceived than carried out, and we have now to state the results of the practice, for it is our duty to record everything remarkable, bad or good. We pass no comment at present, except to express our surprise that so little distress was caused by the presence of the fluid in the lung.

Before relating the cases which we have marked for extraction, we may say that the subject has been the occasion of much dispute in the New York Academy of Medicine, and that a committee appointed to report upon it has sent in a divided verdict—the majority reporting against Dr. Green, the minority reporting in his favour. These reports are to be found at full in the *American Monthly Magazine*, for January and July 1855.

The cases are as follow :

**CASE 1.**—Early in December, John B. Miner, professor of law in the University of Virginia, came to New York for medical treatment. He was accompanied by his friend and colleague, Dr. Davis, the distinguished professor of anatomy of the University of Virginia, by whom, in connection with Dr. Cabell, Professor Miner had been treated.

I saw him first on the 4th December, 1854. Enfeebled by the journey, Professor Miner was unable to leave his room for a week after his arrival in New York. It will not be necessary to detail minutely the previous history of the case. From the patient, and Dr. Davis, I learned that symptoms of thoracic disease, following chronic follicular disease of the pharynx, made their appearance nearly a year before. A severe cough, with debility, emaciation, and occasional hemoptysis, were the rational signs most prominently manifested in his case.

The following was observed to be the condition of the patient at the first examination. There is dulness in percussion at the apex of the right lung. During inspiration, the upper part of the right chest expands less than the left. Expiration, on this side, is prolonged, whilst the respiratory murmur is increased in force under the left clavicle. The “clicking rônchus” of Dr. Cotton, which is positively indicative of the existence of tubercles, is not observed, but numerous bronchial râles are heard on both sides. A severe cough, with large muco-purulent expectoration, which is occasionally streaked

with blood, is present. The patient is very feeble, with loss of appetite, and a voice partially aphonic. Inspection of the throat revealed evidence of long-continued follicular disease; for the mucous crypts of the pharynx had disappeared, and the right tonsillary gland was entirely destroyed, and its place between the anterior and posterior columns was occupied by a large, deep ulcer. On looking into the throat this ulcer was entirely concealed by the anterior column, until this fold of the membrane was pushed aside by the finger. The uvula was elongated. At the request of Dr. Davis, and in consultation with him, I commenced the treatment of the patient. The elongated portion of the uvula was removed; applications of a strong solution of the nitrate of silver were made to the ulcerated portion of the throat and the pharynx, and the iodide of potassium, in combination with minute doses of the proto-iodide of mercury, was administered internally.

At the third application of the nitrate of silver solution, the sponge probang was passed into the larynx, and these operations were repeated daily until the 11th December. Under this topical medication the ulceration in the throat was healed, and the acute sensitiveness, peculiar to the opening of the glottis, was allayed. The cough was, also, to some extent, diminished; but this symptom was still severe, and the bronchial expectoration, and other thoracic symptoms, remained about the same as at first.

Confident, from the results which had followed the treatment in other similar cases, that the introduction of a large amount of the caustic fluid into the bronchial divisions would be attended with greater benefit to the patient, I resolved to make the attempt to inject the lungs.

On the 11th December, in the presence of Professor Davis and several other physicians, I introduced number 12, of Hutchings' flexible tubes, through the rima of the glottis, and carrying it down to the right bronchial division of the trachea, I injected, with a small glass syringe, one and a half drachm of a solution of nitrate of silver, of the strength of thirty grains to the ounce of water, through this tube into the lung.

This operation was performed without producing any cough, except at the moment of the introduction of the tube into the opening of the glottis; nor did any feeling of suffocation, or any irritation whatever, follow the introduction of the solution into the chest.

On the 12th, the operation was repeated, and the same amount of the caustic solution was injected into the bronchial tubes. On the 14th and 15th, the larynx and trachea were cauterized by the application of the sponge probang to those parts, and on the 16th, the tube was again inserted, and the bronchial divisions injected with nearly two drachms of the argentine solution. The cough and expectoration of the patient now diminished much more rapidly than when the probang only was employed. His appetite was restored, and his strength and general health improved daily. This operation of catheterism of the air tubes was continued until the 25th of the month, when Professor Miner considered himself sufficiently restored to health to return to his home, and resume his duties as lecturer in the university. He had in this time gained several pounds of flesh; his cough and expectoration, which had harassed him for months, had disappeared, and from an enfeebled condition, which prevented him from walking the distance of one block without assistance, he had regained so much in strength and vigour, that for several days before he left New York he walked daily two or three miles without fatigue or inconvenience.

But what is equally interesting and important is the fact, that in an examination of the patient's chest, on the day of his departure for home (and this examination was made not only by myself, but by several good auscultators),



it was found that the physical signs which were present at first had quite disappeared.

Only a day or two ago, I received a letter from Dr. Davis, in which he writes, "It will be gratifying to you to know that Professor Miner has not been compelled to suspend his lectures, or to omit his daily exercise since his return," for a single day.

CASE 2.—James Moore, of New York, æt. 35, came under treatment September 24th, 1853. In September, one year before, Mr. Moore began to lose flesh, debility, with a slight cough, soon came on. He had suffered occasionally for several years from chronic pharyngeal disease, and enlarged and diseased tonsils. But as this condition of his throat had occasioned but little inconvenience, no particular attention had been called to it until symptoms of thoracic disease made their appearance.

These continued to increase during the winter and spring of 1854. A severe cough, with subsequently a free, muco-purulent expectoration, constant emaciation and debility, were the prominent symptoms in his case; symptoms which gradually augmented in severity until the above period, the 24th of September, when he came under my care. At this time the rational signs which his case presented were those above named, together with dyspnoea, on exertion being made, and partial aphonia.

The physical signs were correspondent. Dulness on percussion, with crepitating râles were observed over a part of the right lung. Near the upper portion of this lung, strongly marked signs of a tuberculous excavation were present. These physical signs were observed by several good auscultators.

The throat of the patient was in a diseased condition. The uvula was elongated, the follicles of the pharynx were inflamed and enlarged, and full of ulcerated openings, from which purulent matter exuded.

The diseased portions of the enlarged tonsils were removed, the uvula shortened, topical applications of the nitrate of silver were made to the pharynx, and were soon carried into the larynx and trachea, and the iodide of potassium, with tonic and supporting remedies, were administered.

This plan of treatment was continued (the applications being made twice and three times a week) until early in November, varied, so far as the general treatment was concerned, as circumstances seemed to indicate. During this time the patient made some improvement; his voice was restored, his cough was in some degree lessened; but the amount expectorated in the twenty-four hours remained about the same, and it was remarked that the patient continued gradually to emaciate. After consultation with some of my professional friends with regard to his case, it was concluded to employ catheterism, and to carry the point of the injecting tube, if possible, into the right bronchial division. This operation was performed first on the 13th of November, and nearly two drachms of the argentine solution injected into the right lung.

This operation was repeated once in two or four days, alternating the tube with the sponge probang, until the 15th of January. Within twenty-four hours after the first injection, both the cough and the expectoration of the patient began to diminish. He soon commenced to regain flesh and strength, and every unfavorable symptom continued gradually, and in comparison with what had previously occurred, rapidly to diminish.

On the 6th of January, along with my colleague, Professor E. H. Parker, I made a careful examination of the patient's chest. The respiratory murmur could be heard full and clear on both sides; prolonged expiration in one location was the only abnormal sign present.

January 25th, Mr. Moore called again, and reported himself "quite well." He has no cough or expectoration, except some slight raising in the morning. He is quite strong and hearty, can walk any reasonable distance, and attends constantly to his ordinary business.

CASE 3.—In December last, Dr. Peck, of Circleville, Ohio, called on me, and stated that he had accompanied from Ohio, at the request of her husband, Mrs. S. N. Adams, to New-York, for the purpose of consulting me in regard to her case. An appointment was made to see the patient the next day. On the 25th of December, in company with her physician, Dr. Peck, I examined Mrs. A.'s case. She had had long-continued folliculitis of the pharyngo-laryngeal membrane, with enlarged and diseased tonsils.

Auscultation revealed signs of extensive bronchitis, with pulmonary emphysema. Slight dulness, which was found on percussion, immediately under the right clavicle, with rough respiration, gave indications of the commencement of tubercular excavation in this locality.

The patient was feeble and emaciated. She had a hard cough, with constant dyspnœa, and large muco-purulent expectoration. But the most troublesome and harassing feature of her complaint, was the occurrence nightly of a severe and distressing attack of spasmodic asthma, so severe as to deprive her entirely of sleep during the whole night. It was only after the appearance of daylight, by being supported in a sitting posture, that a brief period of repose could be obtained. These attacks had continued to occur for several months, every night, and with great regularity. All the ordinary remedies, Dr. P. informed me, had been employed in the management of the case, without obtaining any material alleviation of the symptoms.

In commencing the treatment of Mr. A.'s case, the enlarged and diseased portions of the tonsillary glands were removed; applications of a strong solution of the nitrate of silver were made daily to the pharyngo-laryngeal and tracheal membrane. The iodide of potassium, in a decoction of polygala senega, together with anti-spasmodics, was internally administered. The cough and expectoration were somewhat diminished under this treatment, but the periodic attacks of asthma were in no degree relieved.

On the 4th January, instead of employing the sponge probang, the elastic tube was introduced, and one drachm and a half of the nitrate of silver solution injected into the bronchi. These operations with the tube, alternating them with the use of the probang, were continued until the 15th of the month, when the patient left the city for her home in Ohio. After the second operation of catheterism, in Mrs. A.'s case, the severity of her symptoms was considerably diminished. Her cough, expectoration, and difficulty of breathing, were all improved, and several nights before leaving the city, she slept quietly all night without any return of the paroxysms of asthma. As she was now anxious to go to her family, it was deemed advisable that she should leave, and that Dr. Peck, who had remained during this time in New York, and had observed her treatment, should continue it as long as necessary after her return to her home.

*Burials in Charcoal.* By B. W. RICHARDSON. ('Journal of Health,' June and September, 1855.)

The present system of burial is attended with two serious drawbacks. It entails expense which can be ill borne by many, and which is a heavy tax upon the poor, and it does violence to the wish which the poor and rich alike experience of being able to sleep at length

among the ashes of those dear to them. These are serious drawbacks to the present system of burial, and therefore we are glad to meet with a suggestion of Dr. Richardson's, which promises to obviate them. This is to enclose the corpse in charcoal before burial. This idea, which arises very naturally out of the recent investigations of Dr. Stenhouse ('Abstract' xxi. p. 1), requires only to be mentioned to be appreciated; for charcoal, as is well known, has the power of hastening decomposition, and of destroying the offensive and noxious products of decomposition. Buried in it, indeed, there is no reason why the dead should not rest within the shadow of the church, even in towns. The expense, also, is very inconsiderable, for it is calculated that a layer of pounded charcoal, four inches in thickness, would be sufficient for every purpose. We thank Dr. Richardson for the suggestion, and shall be glad to hear more about it.



## II.

### REPORT ON THE PROGRESS OF SURGERY.

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*Report on Dislocations, with especial reference to their results.* By Dr. FRANK H. HAMILTON, of Buffalo. ('Transactions of the State Medical Society of the State of New York,' 1855.)

THIS report is a very laudable attempt to supply information which is not to be met with in systematic works on surgery, but which, nevertheless, is of great practical moment. This information is upon the results of treatment. These results, there is reason to believe, are much less favorable than is generally supposed, and it is well, therefore, that the surgeon should be upon his guard, and not promise more to his patient than the case really warrants. This investigation, moreover, promises to be beneficial by pointing out the results of particular modes of treatment, and by thus showing which mode is to be preferred above others.

In the present report, Dr. Hamilton furnishes the notes of a great number of cases which have come under his notice, directly and indirectly, during the last twenty-one years, and much praise is due to him for the careful manner in which he has carried out his object. It is necessary, however, to wait for additional reports of the same kind before we can draw any comprehensive conclusion; and at present we must content ourselves with saying that the evidence, so far as it goes, is to show that the results of dislocations and fractures are more unfavorable than we might have been disposed to suspect. We hope Dr. Hamilton will continue his inquiries, and that other surgeons will not be slow to imitate his example.

*Some cases of Fractures and Dislocations in which it appeared expedient to divide the Tendo-Achillis for the purpose of replacing the separated bones and retaining them in position.* By EDWARD COCK, Surgeon to Guy's Hospital. ('Guy's Hospital Reports,' 1855.)

The principle which suggested tenotomy to the orthopædist for the cure of distortion has been successfully applied in aiding the reduction of dislocation, and in preventing the two ends of a broken bone from being displaced. Two cases are related in the 'Medico-Chirurgical Transactions' for 1850, in which the tendo-Achillis was divided for fracture of the leg—one by Mr. de Morgan, and the other by Mr.

Shaw; and since this time a similar practice has been frequently adopted in the Middlesex and in Guy's Hospital. No doubt the introduction of chloroform very greatly reduces the number of cases in which any such operation can be required for the reduction of a dislocated bone, for chloroform effectually removes the difficulties arising from muscular contraction; but there are a great number of cases in which the bones cannot be kept in their proper place, and to which the practice in question seems to offer the most rational mode of treatment.

Mr. Cock relates several cases which have occurred in his own and his colleagues' practice, and we give the former, leaving them to tell their own tale:

CASE 1.—*Dislocation of Ankle forwards*.—T. B——, æt. 16, was admitted into Luke Ward, December 30th, 1852. About a week previously, he had dislocated the tibia and fibula forwards on to the instep, and although reduction had been effected two or three times by an excellent surgeon in the country, yet the luxation was as often reproduced, apparently by the action of the gastrocnemic muscles.

This fact I verified soon after his admission, and considered it a fit case for division of the tendo-Achillis. The section was made; after which the bones were readily restored, and retained in the normal position without difficulty. The patient left the hospital in about two weeks, perfectly well.

CASE 2.—*Fracture of the Leg*.—R. S——, æt. 64, was admitted into Accident Ward, January 8th, 1853, a cabman, of most intemperate habits. Both bones of the leg were broken a little below the knee. He proved most refractory and unmanageable, and delirium tremens came on a few hours after his admission. Every means employed failed in confining his leg, as he seemed to derive a singular satisfaction in drawing it up, until a considerable angle was formed at the seat of fracture, while the upper fragment threatened to pierce the skin. As all remonstrance proved unavailing, I divided the tendo-Achillis on January 10th; two days after his admission. He seemed to experience much disappointment at being no longer able to displace the bones; and although the delirium continued several days, the fracture was kept tolerably quiet, and he finally left the hospital with a good leg.

CASE 3.—*Dislocation of the Ankle*.—T. R——, æt. 52, a farmer's man, at Epsom, was admitted into Cornelius Ward, January 8th, 1853. He had fallen the same morning from a load of barley, and dislocated his right ankle.

Mr. Stillwell and Mr. Shelley, of Epsom, had used every possible means of extension and manipulation to reduce the foot, without effect. When I examined him, about fourteen hours after the accident, I found the right foot completely dislocated inwards. The sole of the foot faced inwards; the internal malleolus, which was probably broken, lay buried, and could not be felt. The fibula was entire, and the outer malleolus was most prominent, and threatened to pierce the skin. The distortion, increased by the tumefaction, was very great, and precluded the possibility of detecting any fracture which might have occurred to the tibia or astragalus. The foot was perfectly immoveable, and refused to yield to any extension in any direction. The tendo-Achillis was exceedingly tense, and he complained of pain in the calf of the leg. I now divided the tendon, and again had recourse to the same manipulations as before, but with better success, as the foot now yielded to the force employed, and became straight. I could not, however, entirely

complete the reduction, as I found it impossible to bring the malleolus externus into accurate apposition with the corresponding articular surface on the outer side of the astragalus. I believe that the peronei tendons had been torn from their attachment to the lower extremity of the fibula, and had assumed a position on the inner side of the malleolus externus, so that they intervened between the two bones, and prevented their approximation. In a few days, however, the position of the parts had greatly improved, and there was but little abnormal projection of the outer ankle. He partially recovered the use of his foot, and left the hospital about the end of February.

CASE 4.—*Fracture of Leg*.—D. U——, æt. 12, was admitted into Cornelius Ward, April 13th, 1853. I have no memorandum as regards the nature of the accident, but both bones of the leg were broken just above the ankle, and the foot was turned inwards, presenting somewhat the appearance of a dislocation at the ankle-joint. The distortion was very great and remarkable, the upper portion of the tibia projecting nearly through the skin. The displacement would not yield to any amount of extension even when employed while the patient was under the influence of chloroform. I then divided the tendo-Achillis, which was tightly stretched, and repeated the extension by the aid of a round towel attached to the foot. The bones were now replaced with comparative ease, and the case progressed and terminated favorably.

CASE 5.—*Fracture of the Leg*.—J. T——, æt. 58, was admitted into Accident Ward, March 9th, 1853. He had fallen from a height, and fractured both bones of the leg about one third below the knee. On his admission, the tibia presented a considerable angle which projected forwards. All attempts to straighten the limb proved fruitless. There was much general muscular spasm, but the chief impediment to the reduction of the bones appeared to be the tibialis anticus, the tendon of which was unusually large, and so tightly stretched as to keep the foot in a state of extreme flexion. I divided this tendon, and the broken bones were then adjusted without much difficulty. The termination of the case was perfectly successful.

CASE 6.—*Compound Fracture of the Leg*.—J. D——, æt. 26, was admitted into Luke Ward, April 20th, 1853. His left leg had been extensively lacerated, and the tibia comminuted about one third below the knee, by a railway accident. The fibula was broken just below its head; the displacement of the bones was very great; and the constant spasmodic action of the muscles foiled our attempts at adaptation. Under these circumstances, I divided the tendo-Achillis, which had the immediate effect of quieting the limb, and allowing a tolerable adjustment of the fracture.

He subsequently died of phlebitis.

CASE 7.—*Compound Fracture of the Leg*.—F. L——, æt. 42, was admitted into Accident Ward, April 8th, 1854. A slab of stone had fallen on his left leg, and broken both bones about one third above the ankle, with much general contusion and injury. The upper portion of the tibia projected through the skin on the fore and inner part. The fracture was reduced, and the limb placed on the outer side, but the disturbance produced by the action of the gastrocnemius muscle continued to be so annoying, that I divided the tendo-Achillis on the fourth day after the accident. No farther difficulty was experienced, and the case did well.

CASE 8.—*Fracture of the Leg*.—H. S——, æt. 20, was admitted into Accident Ward, April 29th, 1854. Both bones were broken about the middle of the leg; the lower fractured end of the tibia was thrown forward, just piercing the skin, and resting on the anterior surface of the upper portion. There was much effusion and ecchymosis: the broken bones could not be



brought into apposition, and on May 4th, five days after the injury, I divided the tendo-Achillis. A much better position was then obtained, and the patient ultimately recovered, with a tolerably straight leg.

CASE 9.—*Dislocation of Ankle*.—J. H——, æt. 41, was admitted into Accident Ward, July 22d, 1854. He had fallen, and dislocated his ankle. The foot was turned outwards; the tibia had been thrown off the astragalus, and the malleolus internus projected inwards, so as nearly to pierce the skin. There was considerable tumefaction. The usual means for reduction were adopted, under chloroform, without success. The next day, I divided the tendo-Achillis, when reduction was effected without difficulty.

CASE 10.—*Dislocation of Ankle*.—J. G——, æt. 50, was admitted into Accident Ward, August 15th, 1854. He was intoxicated at the time of his admission, and, not being watched, got out of bed and attempted to walk. When I saw him for the first time, on the following day, I found a dislocation of the tibia inwards and forwards; the foot being partly twisted round. There was much swelling, and he suffered great pain. All attempts at reduction failed until I divided the tendo-Achillis, after which, by gentle traction, the bones were gradually restored to their place. He left the hospital well in a few weeks.

In all the cases I have enumerated, the division of the tendo-Achillis appeared to be perfectly innocuous at the time, and devoid of any after mischievous consequences.

*On certain consequences of dividing the Tendons of contracted Muscles.*  
By JAMES BRAID, Esq., of Manchester. ('Association Medical Journal,' September 14, 1854.)

In the same paper in which we find the application of hypnotism to the treatment of hysterical paralysis or spasm, we find the statement of certain facts, which are not sufficiently understood or appreciated by orthopædic surgeons. Mr. Braid first directed attention to them in 1841. "It is a curious fact," he says, "that a rigid and permanent contraction of one or two muscles may be sufficient to exhaust the whole nervous and muscular energy of the leg or arm where it occurs, so that there shall be a loss of heat, feeling, and muscular energy of the whole members, all of which may be restored, with so much rapidity, by simply dividing the tendons of the contracted muscles, that it would be no difficult matter to make superstitious people believe that a miracle had been performed. Every one who has treated cases of club-foot must have observed the increase of temperature which follows the operation; but, in cases of contraction of the hand, and fingers, and arm, from any contraction of the biceps, long flexors of the wrist and fingers, &c., the division of their tendons has, in many instances, been followed by the most surprising results. Not only have the hands and fingers been set at liberty, but such increased energy has been restored to the whole arm, that patients of upwards of thirty years of age, who had been deprived of the power of lifting their arms for five or six or more years, have, in the course of two or three minutes, been able to lift them to their heads. In one case, a patient, upwards of seventy years of age, who had paralysis of the right side, and had been dumb for three or four years, in

eight or ten minutes after the operation, was able to lift the arm pretty freely, and the following morning spoke for the first time since his paralytic seizure.

“To maintain this advantage, so soon as the cutaneous wounds have closed, and lymph has been effused to unite the divided ends of the tendons, it is necessary to make extension, so as to insure a new portion of tendon growing into the point of separation, otherwise, on the reunion, contraction might be re-established, and with that the original feeble state of the member.

“The only physiological reason which I can offer, as to the cause of the restoration of power and feeling after these operations, is this. I infer that a certain amount of nervous energy is elaborated for each member for the general supply of the whole of its functions, and that the quantity necessary to maintain these muscles in this state of morbid tension produces enervation of all the others, with loss of tone, and languid circulation. The operation, by cutting off this profuse expenditure in one channel, allows it to flow for the general benefit of the whole, and hence the muscular power is restored, with freer circulation of blood, which will, of course, produce an increase of heat in the whole member. The operation is also frequently followed by increased vigour of the whole body. This was remarkably evinced in a boy on whom I operated for talipes, from contraction, in 1840. He was ten years of age, paralytic of the leg, so that he had never had the power either of standing or walking on it, and so weak in his body generally, that he was a most pitiable object, leaning feebly over his crutches, unable to support his head erect. In three weeks after the operation, he was able to walk across the floor by the help of supporting him by the hands. In a short time longer, he could walk with his crutches, supporting most of his weight on the leg, and supporting the head with ease; and ere long he was enabled to throw his crutches aside entirely, and walk with a little stick in his hand.”

We cannot accept the explanation, but we state the fact, and a case in illustration.

CASE 1.—On the 3d of March, 1854, I was consulted in the case of a boy, 3 years and 9 months old. His father was a strong healthy man; his mother had been delicate ever since her second confinement. She was the mother of four daughters and four sons, and all the four sons, of whom my patient was the youngest, were born at the seventh month. My patient had a well developed brain, and seemed very intelligent for his age; but he had never had the volitional control of either legs, arms, or fingers. He had never been able to pass his arms round any one nursing him, so as to aid in supporting himself, nor to lay hold of, or pick up anything with his fingers, or fingers and thumb. When he attempted to grasp hold of anything, it was a mere clutch with the whole fingers together, and he had no power of loosing his hold but in the same manner. As regarded the inferior extremities, from tonic spasm of the internal iliac and psoas muscles, the thighs were permanently and rigidly flexed on the trunk; and, from a similar state of the adductors of both thighs, the knees were held rigidly in close contact. Moreover, from rigid tonic spasm of the flexor muscles of both legs, they were held strongly flexed on the thighs; and, from tonic contraction of the gastro-

cnemii muscles of both legs, the heels were drawn up, and the toes and feet held extended, in the form of a complete case of talipes equinus. The boy was thus as miserable an object as could well be imagined, entirely deprived of the use of both superior and inferior extremities. Several professional gentlemen had been consulted in this case, but without having afforded him the slightest relief. At length, one professional gentleman, personally unknown to me, recommended the parents to bring him to me, from knowing that I had given a considerable share of attention to the treatment of various distortions.

By the hypnotic process I was enabled to excite the antagonist classes of muscles into play, and thus reduce the morbid activity of those which had bound the limbs in this state of permanent rigid contraction; so that in about ten minutes I was enabled, with very little effort, to extend and separate his legs to an extent which never could be achieved previously by the strongest efforts which any one had applied to them. This same process was repeated with equally satisfactory results on two subsequent days; but it appeared to me that the far most speedy and certain mode of effecting a cure in such a case would be at once to divide the tendo-Achillis in both legs by subcutaneous section, and thereby ingraft a portion of new substance between the divided ends of the tendons, as practised in treating ordinary cases of talipes equinus. By this means I not only expected to restore the feet speedily to the normal position, but also that there would be a decided improvement effected on many of the other morbidly contracted muscles, through the laws of sympathy and consensual action, which plays such an important part in nervous power and muscular motion. To this proposal, the parents readily gave their assent, and I operated accordingly on both legs, on the 8th of March, 1854. The small punctures in the skin, made by the narrow blade of the tendon knife, having been closed by adhesive plaster, I applied splints and bandages, so as to retain the feet in their original malposition, and compress the gastrocnemii muscles, so as to prevent such separation of the ends of the severed tendons as might prevent the lymph thrown out extending to both ends, and thereby preventing their ultimate reunion. In two days the splints and bandages were removed and reapplied; but so as to secure slight extension, and a farther effusion of lymph, to increase the bond or medium of reunion. This extension was increased at each dressing, so that, in fourteen days there was sufficient addition of substance implanted between the divided ends of each tendo-Achillis to permit the feet to assume the natural position to the legs, and sufficiently strong to warrant me in removing the splints and bandages, and providing the patient with a pair of lace-up leather boots, with side splints. Having put on the boots he was found able to stand with his feet flat to the floor, and to support a considerable portion of the weight of his body by his own muscular efforts.

I had also applied gutta-percha splints behind the legs and thighs, supported bandages, so as to assist in overcoming the contraction of the flexors of the legs. These were kept constantly applied night and day for a month, and, after that period, during the night only for ten weeks, after which they were entirely dispensed with. At each dressing, moreover, I manipulated, as in the first instance, so as to excite the morbidly weak muscles, and to subdue the morbid rigidity of the others. I also induced the patient to exert his best efforts at walking during my visits, and the result was that a considerable increase of power had been attained by him at the end of April; and, on the 2d of May, I sent him into the country for the benefit of air and exercise. He remained in the country till the beginning of August with decided advantage. Since his return home, I saw this patient a few times,



when I have manipulated and exercised him as I did prior to the division of the tendons, which manipulations seemed also to be highly beneficial to him.

There is one remarkable fact connected with this case, to which I beg now to direct your special attention, in corroboration of what was remarked at the commencement of this paper,—viz., that in about six-and-twenty hours after the section of the Achillis tendons,—the patient was discovered to have acquired complete volitional power of his arms, hands, and fingers, for the first time during his life; and the most gratifying part of the narrative is this, that his volitional control has remained perfect ever since; thus clearly proving the remarkable consensual and sympathetic influence of one part of the muscular and nervous system with that of others.

In the same paper, Mr. Braid relates several cases, in which he cured relaxed and paralysed muscles by excising a portion of the tendons, and by bringing the divided extremities together and securing their adhesion in this position. In these cases, also, the division of the tendon seemed to stimulate the muscle and the system generally to some degree of higher activity—and the fact is well worthy the attention of surgeons—but our present purpose is merely to direct attention to the collateral effects of the Stromeyerian operation—effects which show that the immediate effect upon the muscle is only part of the benefit derived from the operation.

*On the treatment of Dislocation of the Hip by Manipulation only.* By W. W. REID, M.D., of Rochester, New-York. ('New-York Journal of Medicine,' July, 1855.)

In this paper Dr. Reid criticises at considerable length the paper of Dr. Markoe upon the same subject ('Abstract,' xxi, p. 315), and in stating his own views more clearly, corrects some errors into which Dr. Markoe has fallen.

Dr. Reid deduces the following proportions and rules from his experiments and observations:

"1. The chief impediment in the reduction of dislocations is the indirect action of muscles put upon the stretch, by the malposition of the dislocated bone, and not in the contraction of muscles that are shortened [as heretofore taught].

"2. That muscles are capable of so little extension, beyond their normal length, without hazard of rupture, that no attempt should be made to stretch them further, in order to reduce a dislocation, if it can possibly be avoided.

"3. The general rule for reducing dislocations should be, that the limb or bone should be carried, flexed or drawn in that direction which will relax the distended muscles.

"This general rule will apply to all luxations, but especially to the several varieties that pertain to the hip-joint."

Again, in another place:

"The method of manipulating, as employed and described by me in the article published in the 'Buffalo Medical Journal' for August, 1851, and again in the proceedings of the State Medical Society, February,

1852, was as follows:—Let the operator stand or kneel on the injured side, seize the ankle with one hand, the knee with the other, then flex the leg on the thigh, next strongly abduct it, carrying it over the sound one, and at the same time upward over the pelvis, by a kind of semi-circular sweep, as high as the umbilicus; *then abduct the knee gently*, turn the toes outwards, the heel inwards, *and carry the foot across the opposite and sound limb, making gentle oscillations of THE THIGH*, when the head of the bone will slip into its socket.

“‘Gentle oscillations of *the thigh*,’ while the head of the bone is poised upon a mere point, as before described, and while the foot and leg are directed towards and across the opposite limb, and steadily held in that position, will be found, I apprehend, to be ‘a procedure that varies a little’ from that recommended by Dr. Markoe, viz., a rocking motion of *the leg*, while the thigh is being brought to the straight position *strongly abducted*.

“When the thigh is flexed on the trunk, say at an angle of  $45^{\circ}$ , and is gently abducted, and the head of the bone thus brought close to the lower edge of the acetabulum, if, while gentle oscillations of the thigh are made at the knee, it—the head—does not immediately enter the socket, the knee should be alternately elevated and depressed, thus varying the angle of the thigh. If, by this manœuvre, alternated with the beforementioned oscillating or lateral movement, the head does not enter, we should then cease all motion, and hold the thigh and leg perfectly quiet, for a short period, keeping the former still slightly abducted; and thus give the irritated muscles, ligaments and tissues time to become quiescent, and to accommodate themselves to the new position of the bone. The foot and leg must be kept still also, and firmly directed towards the opposite thigh; for, if we relax or carry it outward, we shall roll the head of the femur away from its resting place and proximity to the acetabulum, and permit, if not provoke the muscles, as already described, to draw it downward into the foramen ovale or backward into the ischiatic notch or dorsum ilii. After a short time we may repeat our attempts as above described, and in all suitable cases—that is—cases of dislocation on the dorsum or into the ischiatic notch, and of not over four to six weeks’ standing, we may confidently anticipate a speedy and favorable issue.

“The accidents which occurred during the manipulations of Dr. Markoe and his coadjutors, with the exception of one, in Case 14, were by no means serious; they were in fact discoveries, and afford us valuable lessons and suggestions. It is certainly something new in the annals of surgery, that a surgeon can, at his pleasure, convert any one of the four kinds of dislocation of the hip-joint into any one of another kind. With this additional knowledge, which furnishes us with a choice of position from which to attempt reduction, or, if one fails, to try another, it seems to me, that these heretofore most formidable luxations of the femur may hereafter become, when the *modus operandi* by manipulation is better understood, quite trivial affairs, and be reduced with as much, if not greater facility, than now obtains in reduction of the shoulder joint.”

It is of extreme importance to follow out the exact rule in placing and moving the thigh, so as to gain the proper help of the muscles in

effecting the reduction, and to enforce this position, Dr. Reid refers to a statement by Professor Moore on this subject.

“Dr. Moore states a fact, which I had before observed, that when the muscles about the joint are entire, if the femur is flexed on the trunk and abducted, the glutei muscles are stretched, and then their broad tendons compress the trochanter, and powerfully assist rotation and abduction, to urge the head of the bone into the socket; very much as might be done by the hands, if placed over the trochanter, but much more efficiently. But if the thigh is brought down to a right angle or lower, the tendons of the glutei are relaxed, and we thereby lose all the advantage which they would otherwise afford us—another cogent reason for not bringing down the limb towards the straight position, before the head enters the socket.”

Dr. Reid also thinks that Dr. Markoe may have failed in some of his cases in consequence of having subjected the patient to etherization, for it is possible that the ether may have relaxed the muscles and so prevented that co-operative action of which mention has just been made.

“One word on the use of ether and chloroform in reduction of luxations. Believing, as I do, that muscles are the principal agents in producing dislocations, while the blow received is but secondary—that is, that the muscles being in a state of active contraction by force of the will, at that instant a sudden blow, taking the will by surprise, is received on the limb, and propelling it in the same direction which the contracting muscles are giving it, the bone is thrown out of place before the opposing muscles have time to resist this new impulse. I am therefore constrained to believe, also, that muscles may be made, and are, in fact, our most efficient assistants in reducing by manipulation. Consequently ‘etherization to the extent of complete relaxation,’ instead of being an advantage, is a detriment, in just so far as it prevents the contraction of the muscles required to replace the bone. Might not this have been one of the causes of so many unsuccessful attempts at reduction, which are narrated in the report of Dr. Markoe? Etherization was employed in every case reported as occurring in the hospital, except No. 5, and that was the only one which was reduced without difficulty or mishap. The pain produced by manipulation is too trifling to require an anæsthetic of any kind. These remarks, of course, are not intended to apply to cases of luxations complicated with other severe or painful injuries, nor to reduction by forcible traction or mechanical means.”



### III.

## REPORT ON THE PROGRESS OF MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN.

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*Clinical Lectures on the Diseases of Women and Children.* By GUNNING S. BEDFORD, A.M., M.D., New York. (Trübner and Co., Paternoster Row, London, 1855.)

ALTHOUGH we are indebted to America for the application of anæsthesia to therapeutics, and for exhibiting to us the almost inexhaustible powers by which nature sometimes recovers patients from operations to us appalling by their boldness and magnitude, still we may safely say there is no American school of medicine; whereas there is a French, a German, an Italian, and an English. Our Transatlantic offspring reprint, translate, and pirate the medical works of other nations, but they produce little of their own. Their pathology is chiefly French, their therapeutics English.

In so vast a country, there doubtless are many eminent medical men, and some whose works are valued, but even in most of these we can detect the national characteristic—the impulse to go ahead. The working men in America are always on the look-out for the new lights rising over the old world, and they often too hastily adopt, as the pure gold of science, the crude lucubrations which must find place, with more valuable matter, in the weekly medical press. But this observation does not apply to our author, who is creditably known by other works, and in the one before us shows himself to be a judicious physician, anxious alike for the good of his patients and of his pupils; one who has acquired the happy art of teaching how to get at the characteristics of disease, and how to drag at the chain of effects until the mind grasps the first link in the chain. The work before us would, however, it seems to us, be more useful to the American medical student, had cases of the same disease been placed side by side, so as to light each other and facilitate introductory remarks. Instead of this, similar cases are placed at random in different lectures, and not unfrequently are the most interesting cases cut in two, the halves being separated by thirty pages of letter-press. This might easily be corrected without altering the form of the lectures. Not quite so easy, however, would it be to make another alteration, which would require the work to be almost rewritten. We allude to the singular conversations introduced, between the doctor and his patient, which pervade the book, and for which Dr. Bedford has followed the unhappy example of Dr. Meigs. Now,

though it is often extremely useful to give the precise words by which a patient gives, as it were, the reflected image of what she feels; though the noting down of the question and answer not unfrequently gives the pith of the case; yet to half fill a volume with unnecessary twaddle, real or imaginary, faintly enlivened by an attempt at a witticism or a grand display of professional disinterestedness, is a practice that should be discountenanced as much as possible.

Without further reflections, we shall proceed to let the reader judge of the merits of Dr. Bedford's work by a few extracts, selected as most interesting to the profession, and shall commence with a subject warmly debated some time ago, and upon which opinions are still divided. We allude to "ulcerations of the os uteri." Dr. Bedford admits the frequency of uterine lesions, and lays a stress on the necessity of their local treatment. He dissents from the views maintained by Dr. West in his pamphlet, and thus sums up his own views:

"1. That ulceration of the *os uteri* is of frequent occurrence. 2. That in many instances, this ulceration is little more than a simple abrasion, giving rise to no local or constitutional disturbance, and will readily yield to rest in the recumbent position. 3. That neither the abrasion nor ulceration can be strictly considered primary affections—the former being frequently connected with congestion, whilst the latter is the result of inflammation of the organ. 4. That oftentimes simple ulceration, unattended by any structural change in the uterus, will not develop either local or general disturbance of the system. 5. That in many cases ulceration of the os requires judicious local treatment. 6. That with the local applications there must often be conjoined constitutional measures. 7. That the disturbances of the general system dependent upon either ulceration of the os uteri, or other derangements of the organ, will cease with the removal of these derangements. 8. That constitutional disturbances are often referred to ulceration of the os uteri, when no ulcerations exist—but in lieu of which there is some functional or organic disease of the uterus."

Occlusion of the os uteri is so rarely met with, that its occurrence three times in a short period deserves notice. In discussing a case of this description, the author says:

"What does this woman tell us, and what is the real point elicited by the conversation to which you have just listened? It is simply this—that she is twenty-six years of age, the mother of two children, in the enjoyment of good health until two years and four months ago, when she had a miscarriage, followed by inflammation of the womb, *since which time her courses have been suppressed*. The feature, then, of this case is the suppression of the courses—but I shall prove to you that, in regarding the suppression in an abstract point of view, and attempting upon this partial basis to restore the function, you would not only fail in the accomplishment of the object for which your remedies are administered, but you would aggravate the sufferings of your patient, and lapse into positive empiricism.

"In reply to a question addressed to her a few moments since, she says. 'I have taken so much medicine that my stomach is quite turned over;' and again, she observes: 'I have forcing-pains every month, and that's the time the doctor told me to take the medicine; for it would

bring on my turns.' If the declarations of the patient are of any value—if they establish any fact, it is this—that the sole object of the doctor was, through the medicines he ordered, to restore the menstrual function, but he has, as you perceive, from the testimony of the patient, failed in the attainment of his purpose. Does it occur to any one of you why he has failed in affording relief to this woman? The entire interest of the case before us is embraced in this simple interrogatory; and its solution will shed a flood of light on the extraordinary circumstances which have caused the interruption of the menses for a period of two years and four months. You have seen in the Clinique numerous cases of suppressed menses, produced by various causes—and you have likewise witnessed how readily they have yielded to judicious treatment. But the suppression in the case of this patient differs materially from that of all others which have been before you; and with a field for observation of no limited circuit, and with a practice of fifty years, you would probably not meet with one similar in its leading features. Those of you who have attentively analysed the conversation which has just passed between this patient and myself, will, perhaps, be struck with the important fact disclosed in the dialogue, viz.: that two years and four months ago she had a miscarriage, *followed by inflammation of the womb, and since that time her courses have been suppressed.* Before introducing this woman to you, I interrogated her very fully, and as soon as she made the above announcement, I begun at once to suspect the cause of the suppression—and I immediately asked her whether she had not an enlargement in the lower portion of the abdomen. On her replying in the affirmative, I told her it would be necessary to institute an examination in order that I might ascertain the true nature of her disease. To this she consented, and the examination has revealed a most interesting and unusual state of things.

“Before proceeding further, however, it is proper that I should tell you the motive and object of my suspicion. 1st. It occurred to me that this might be a case of menstrual suppression from an occlusion of the os tincæ. 2d. This opinion was formed from the circumstance that the suppression commenced immediately after the inflammation of the womb, and has continued to the present time. Supposing my suspicion to be confirmed by an examination, what connection, you may ask, is there between an occluded os tincæ and an enlargement of the lower portion of the abdomen? When the menstrual blood is secreted, and has no outlet, it necessarily accumulates, under ordinary circumstances, from month to month in the uterine cavity, and thus the enlargement is produced. In my lectures on pregnancy, you will not have forgotten how emphatically your attention was directed to this subject, and how earnestly you were cautioned against mistaking, especially in the unmarried, this state of things for gestation.

“But what gives peculiar interest to this case, and constitutes it an exception to a very general rule, is the fact that there is an *imperforate os tincæ in a female, who has borne two children.* The fact of her having given birth to two children necessarily presupposes that the mouth of the uterus was not always imperforate, or, in other words, that the occlusion was not congenital. What, then, has produced the occlusion? The whole history of the case seems to demonstrate that it is the result



of the inflammation with which the patient was affected after her miscarriage. This is the third example of imperforate os tinæ I have met with during the last few years in married women, who had previously given birth to children. In the two former cases, I was called when the patients were in labour, and performed the operation of *vaginal hysterotomy*, and in both instances the mother and children were saved. [The first case was reported in the 'New York Journal of Medicine' for 1843, the second in the 'American Journal of the Medical Sciences' for 1848.] As soon as I had satisfied myself as to the true condition of the patient before us, I requested two of my staff Drs. Martin and Savage, to institute an examination, and thus afforded them an opportunity of testing the truth of my diagnosis. Now, gentlemen, permit me to ask you what is the moral of this case? It is clearly this—that symptoms are not only faithless guides, but lead often to negative, if not to destructive results. Fortunately, in the present instance, the treatment has been limited to a negative issue. The indication here is obviously to remove, by an operation, the occlusion. It is the first step, without it all other medication would be abortive, and purely empirical.

“ ‘Madam, are you aware of the difficulty under which you labour?’ ‘Yes, sir, I have heard you say that I have an obstruction.’ ‘That is a very proper word, my good woman. Do you wish to have the obstruction removed?’ ‘Oh! sir, I would if it is not dangerous.’ ‘There is no danger, madam, if the operation be rightly performed; and if you will consent, I will perform it without any further delay.’ ‘You are sure, sir, it won’t kill me?’ ‘Indeed, I am, my good woman. We do not kill people—our profession is intended to save, and not to destroy human life.’ ‘But, sir, people do die in spite of the doctors.’ ‘Yes, madam, that is true; there is a limit to all human skill, and it sinks into insignificance before the high decrees of Heaven! Will you permit me to relieve you?’ ‘Anything you say, doctor.’ ‘Then, madam, I will do what is right for you.’ [Here the patient was placed on the bed, and the tumefaction of the abdomen in the hypogastric region was clearly visible. The professor observed that he would use the curved trochar for the purpose of penetrating the imperforate os; and, accordingly, taking his index finger as a guide, he introduced the instrument to the central and lower portion of the cervix, and carrying the trochar upward parallel to the long axis of the uterus, penetrated the lower portion of the organ without the slightest difficulty. The instrument was then withdrawn, and immediately Simpson’s sound introduced, showing conclusively that the neck of the uterus had been penetrated. As soon as the sound entered the organ, there was a discharge of nearly a quart of grumous blood, which the professor regarded as the menstrual fluid which had been accumulating within the cavity of the womb.] You perceive this operation is a simple one, and yet it is not without danger if incautiously performed. In order that the os tinæ may be kept open, it will be proper for a few days to introduce a gum-elastic bougie. This is all that will be required. ‘Now, madam, have I killed you?’ ‘No, sir, indeed you have not!’ ‘You may rejoice that your good sense in submitting to this operation will be the means of restoring you to health.’ ‘Oh! sir, I owe my life to you.’ ‘Not at all, my good woman, you owe everything to yourself; I could have done nothing for you if it had not been that you were a sensible woman. Good morning.’ ”

In one of Dr. Bedford's three cases, the occlusion took place in consequence of repeated manipulation to induce miscarriage. No such means had been resorted to in the other cases, no caustic had been applied to the os uteri. The occlusion occurred, then, in consequence of inflammation so severe as to bring about the union of the lips of the womb by first intention. These cases will, therefore, be very interesting to those who admit that the neck of the womb is only susceptible of syphilitic inflammation. There are several valuable cases of hydatids of the womb; and the diagnosis of this complaint is well treated, as follows:

"1st. They may be mistaken for pregnancy. 2d. For polypus. 3d. For physometra. 4th. For hydrometra. 5th. For cauliflower excrescence, &c. The stethoscope, the ballottement, and the active movements of the foetus, will determine the question of pregnancy after the fourth and a half month. In polypus, there is a mucous and bloody discharge, but no discharge of water; the polypus, also, can often be felt through the os tincæ, when it does not project into the vagina. In cauliflower excrescence there is a discharge of water, and when the pellicle, covering the granules, which really constitutes the disease, is ruptured, there is also a discharge of blood; but in cauliflower excrescence, the watery discharge is *continuous and not periodical*, for the reason that it is a secretion from the pellicle, to which we have just alluded. Hydrometra, or dropsy of the womb, is extremely rare; when it exists, the fluctuation will serve to distinguish it from hydatids.

"It is not difficult to explain this discharge of water. Uterine hydatids usually consist of small oblong sacs filled with serous fluid; these sacs are pediculated, and hence have been compared, not inaptly, to a bunch of grapes. They become developed in size, and those which are the most dependent in the uterine cavity, as they increase in volume, irritate the neck of the uterus; this organ is thrown into contraction for the time being; the dependent sacs are ruptured, and their contents, consisting of serum, are discharged through the vagina. The same thing occurs again in proportion as the sacs next in order become developed; and you see, therefore, why it is that the discharge of water in uterine hydatids is not continuous but periodical."

Describing another case of hydatids of the womb, in which the principal symptom had been vomiting, so intense that no remedies could abate it, he says:

"On applying my hand to the abdomen, I found the uterus enlarged, and occupying the hypogastric region. The alarming situation of the patient would not justify delay; if her life were to be saved, everything admonished us that it was to be done by instantaneous measures. My opinion of the case was, that the vomiting was sympathetic, and produced by irritation of the uterus. I therefore suggested the propriety of endeavouring to induce contraction of this organ, in order that its contents might be expelled. This view was concurred in by Dr. Whiting. Accordingly, with the doctor's request, desperate and almost hopeless as the case was, I introduced a female catheter into the uterus; in a short time the organ contracted, and a mass of hydatids was thrown off. Almost immediately, as if by enchantment, the vomiting ceased. The



patient, after a tedious convalescence, from her extreme prostration, recovered, and is now in the enjoyment of robust health."

Flooding is rightly pointed out as the chief danger of the disease; and amongst the well-known means of inducing contraction of the womb, he mentions one of which we have frequently availed ourselves with good effect:

"A capital remedy, too, in such cases, is the introduction of a piece of *ice* into the vagina, carried up to the neck of the organ. This, sometimes, displays magic effects in producing uterine contractions, and upon a principle which has often been explained to you. The excitor nerves of the vagina becoming stimulated by the action of the cold, this stimulus is transferred to the spinal cord, whence an impulse is given to the motor nerves of the uterus, which soon becomes the centre of powerful contractions. On the same principle, ice-water injected into the rectum, or against the mouth of the uterus, is a good remedy under these circumstances."

The following after-treatment is recommended:

"When the hydatids have been expelled, and the patient is convalescent, it will be proper to place her under the action of mercury and sarsaparilla, in order that any occult morbid action in the uterus, and more especially in its mucous lining, may be broken up. With this view, the following course may be pursued:

R Pil. massæ Hydrarg., gr. xxiv;

Pulv. Opii, gr. iij.

Ft. massa in pil. xij dividenda.

"One of these pills to be taken night and morning until ptyalism is produced; after the salivation has been accomplished, let the patient drink half a pint of the compound decoction of sarsaparilla daily, and continue it for a month or six weeks. In the mean time, sexual intercourse should be prohibited. This treatment, together with change of air, sea-bathing, and a nutritious diet, will tend greatly to the restoration of the patient to health."

We are not prepared to endorse this statement until we have further proof of its correctness; but it must be remembered that mercury was so abused during the last century, that we are now, perhaps, over cautious in giving it. The dangers of salivation are not to be put in comparison with the dangers arising from interminable uterine complaints, and the medical man is not to be blamed, who, after trying all other measures, would seek to modify morbid structure by a course of mercury pushed to salivation.

Physometra is a very rare disease. In commenting on an evident case of this description the author observes:

"When tympanites uteri—physometra exists, it is, I believe, in consequence of certain chemical changes, the immediate result of morbid action in the womb itself. A blighted ovum, a retained and decomposed placenta or foetus, or the decomposition of any intra-uterine growth, may result in the extrication of a gaseous fluid, which constitutes the affection before us; and my own opinion is, that this patient is an example of this very cause. You remember the important fact to which she alluded, in reply to my questions, viz.: that the women would



not permit her to see her infant after its birth, for the reason, as we suppose, that it was in a state of decomposition. This is a very interesting circumstance in connection with the case, and, I believe, fully explains the presence of the uterine flatus. Physometra may also result from retention of the menses, or of the lochial discharge. Baudelocque, Lisfranc, and others, have recorded cases of physometra in hysteric women, without having been able to detect any cause for it. May not, in these cases, a secretion of gas have taken place in the womb, such, for example, as occasionally occurs in the stomach of dyspeptic patients?"

Retro-uterine hematocoele is a complaint to which the attention of the profession has been drawn within the last few years by P. Nelaton, of Paris. Dr. Tilt is the only English author who has fully described the complaint in his valuable chapter on sanguineous pelvic tumours;\* and Dr. Bedford gives the particulars of a case of a mild form of this not uncommon disease:

"Mrs. L—, married, æt. 34, the mother of four children, suffers from very severe pain in her back passage, and says she has a frequent desire to have an evacuation from her bowels, but passes very little. 'How long, madam, have you suffered from this pressure on your back passage?' 'For the last two months, sir.' 'How was your health previous to that time?' 'It was always good, sir.' 'Did anything occur two months ago to which you can in any way refer this pressure of which you speak?' 'Nothing, sir, except a fall I had.' 'How did you fall, my good woman?' 'I was coming down stairs, sir, with a tub of water, my foot slipped, and I fell down a whole flight of steps.' 'Were you much injured at the time?' 'No, sir, but I was terribly jolted.' 'How soon after the fall did you begin to feel this pressure?' 'The next day, sir.' 'Were your bowels regular previous to the fall?' 'Yes, sir.' This case, gentlemen, is one about which it is impossible even to approximate an opinion without a minute vaginal examination. Pressure on the rectum may be the result of various conditions, such as retroversion of the uterus, prolapsion of the ovary or small intestine into the triangular fossa, a collection of hardened fæces, internal hemorrhoidal, and other tumours. [The patient was placed on the bed, and the professor proceeded to institute the necessary examination.] From the examination I have just made, it is obvious that the pressure on the rectum, and difficulty in defecation, are owing to a tumour in the fossa, between the intestine and uterus.

"The next question to be decided is, as to the particular nature of this tumour. That it is not the retroverted uterus, I am assured from the fact that the cervix of the organ is rather inclined backward while the fundus is thrown somewhat forward by the pressure of the tumour; and I am equally confident it is not a prolapsed ovary from the two following circumstances, 1st. there is no indication of any disease of either of the ovaries; and, 2dly, if the tumour were occasioned by the descent of a healthy ovary, which sometimes happens, it would be characterised by great mobility, which is not the fact in the case before us. The pressure is not occasioned by a collection of fecal matter, as I

■ 'Diseases of Women and Ovarian Inflammation in relation to Morbid Menstruation, Sterility, Pelvic Tumours, and Affections of the Womb,' 2d ed., Churchill, London.

have ascertained by the introduction of the finger into the rectum. What, then, is this tumour? In my opinion, we have in the person of this patient an example of a most interesting form of tumour—I believe it to be a collection of blood, or what may be termed an hæmatocele: and from the position it occupies is entitled to the name of retro-uterine hæmatocele.

“The reasons for my opinion are these: 1st. The fall would be likely to produce an extravasation of blood. 2d. To the touch, the tumour is soft, elastic, immovable, and evidently contains fluid. If I am correct in this view, a most interesting question arises, What is to be done? Nelaton, in cases like these, recommends the use of the exploring needle in preference to incision, for the reason that there is more or less danger from hemorrhage if incision be had recourse to; and, moreover, he finds that the tumour often becomes absorbed; while, in other instances, the blood escapes through the rectum or the genito-urinary organs. ‘Now, my good woman, if you will permit me, I will ascertain the true cause of your suffering, and will do all in my power to relieve you.’ ‘You may do anything you think best, sir.’ ‘That’s a sensible woman, as full of courage as you are of common sense.’ [The professor here introduced the index finger of the left hand into the vagina, and passed along the finger a small exploring needle, with which he penetrated the tumour, between the rectum and uterus, directing the needle upward. It was evident that the diagnosis was a correct one, for, as the tumour was penetrated, blood escaped.] You see, gentlemen, in the blood which passed from the sac as soon as it was entered, the best evidence of the accuracy of the opinion we had formed touching the nature of the tumour. I do not feel disposed, under the circumstances, to do more than introduce occasionally the needle for the purpose of allowing a small quantity of the blood to escape, for I have very little doubt that this, together with the action of the absorbents, will suffice to disperse the extravasated fluid. It will be proper, however, to keep the bowels in a soluble state, and, in order to accomplish this, I shall direct a pint of tepid water to be thrown up the rectum every night.”

Dr. Bedford seems to have met with vascular tumours of the meatus urinarius more frequently than ourselves, for in large hospital practice we have only met with two cases in eight years; we therefore feel inclined to adopt Sir C. Clarke’s first opinion relative to the frequency of the disease:

“Sir C. Clarke, when he first called attention to it, entertained the opinion that it was of rare occurrence. Subsequently, however, he changed his mind on the subject. My experience tells me that it is not so very uncommon, although I am satisfied it often eludes detection. I have repeatedly met with it, and this is the second case, which has presented itself at the Clinique the present session. This tumour is almost always accompanied by a mucous discharge, and its characteristic symptoms are excessive pain in sexual intercourse, in passing water, and in walking. The contact of the chemise is productive of great suffering. In fine, the slightest touch gives rise to severe pain. Sometimes several of these excrescences will be detected within the urethra.

“*Treatment.*—No medicine which you can administer will have any effect. The only remedy is the removal of the tumour; this may be done

by ligature, the knife, caustic, or scissors. I greatly prefer the latter. Take a pair of curved scissors, and remove the tumour completely, then touch the cut surface freely with caustic. This is all that will be necessary. Occasionally serious hemorrhage follows the removal, which you can check by the nitrate of silver, or caustic potash, together with ice kept constantly applied. It is also recommended to apply to the cut surface nitric acid, being careful to guard the surrounding parts. The actual cautery is sometimes resorted to not only to arrest the hemorrhage, but as a primary remedy, and I should think it an efficient agent for either purpose."

In our cases, after protecting the surrounding parts by lard, we have carefully touched the tumour with concentrated nitric acid. The application was repeated every week for some time, and the patients were ultimately cured.

A much debated question—whether hysteria is marked by unconsciousness?—is thus treated by the author:

"But there is one feature connected with hysteria to which it may be useful to call your attention—it is the loss of consciousness which sometimes supervenes in the attack. It has been seriously doubted whether in hysteria there is ever loss of consciousness, but on what grounds I am sure I cannot understand. To my mind, there is no fact more emphatically established than that women labouring under an hysteric paroxysm do lose for the time being all sense of the external world; while, again, you will find that this want of consciousness is only partial. But there is a circumstance which has been well observed by authors, and which you, too, will recognize in practice, it is that in hysteria the mind does not become lost at the commencement of the attack, but the unconsciousness is always gradual. This constitutes a very important diagnostic symptom between hysteria and epilepsy, in which latter, one of the very first and most prominent symptoms is immediate and complete loss of consciousness. In questioning this girl, you will remember how particularly I interrogated her on this point. My question was, 'Do you always lose your consciousness in these attacks?' She replied, 'Not when the fit first comes on, sir, but I do after some time.' It may, however, be observed, that this derangement of the intellect is not a uniform accompaniment of hysteria.

The preceding extracts are amongst the best passages of a large volume which will doubtless be useful to American students. And though we cannot place it by the side of the lectures of Velpeau, Lisfranc, or Dupuytren, Dr. G. Bedford's reputation already stands on good grounds; and his love of his profession will, without doubt, at some future day, give us the opportunity of awarding unqualified praise to the product of his pen.

*On the Disorders of Infantile Development, and Rickets, &c.* By A. SCHOEPP MEREL, M.D. (Churchill, Burlington Street, 1855.)

The wholesale hospitality we give to the French refugees impelled to our shores by the tyranny of one or by that of many, by an Edict of Nantes or by a reign of terror, has been eminently conducive to



our national prosperity. If, instead of seeking to work mischief, exiles will only exert themselves according to their calling in any useful sphere, they are welcome to take root amongst us, and such men as Dr. Merei will be always welcome. A revolutionary convulsion throws him from Pesth, where he was in good practice, into smoky Manchester. Undaunted by the change, he continues the study of medicine as a science, and its practice as an art, and after a time he writes a book, not, however, to fan the embers of sedition with the hot blasts of revenge, for instead of attacking the Emperor of Austria he judiciously wages war with the causes of Rickets, with the influences contaminating the air of Manchester, with the injudicious feeding of the children of the lower classes, and with their overfeeding by the wealthy, thus giving us a valuable essay on very important subjects. We say *essay* because Dr. Merei only publishes it as an essay, being fully aware that certain portions of the work require to be filled up, but the work is highly suggestive, and deserves an attentive perusal.

With regard to the theory of Rickets, the author adopts Virchow's statements.

"According to this acute and indefatigable observer, all the same vegetative changes are going on in the rachitic bone which in the normally conditioned bone prepare the growth, with this exception only: that in the former no phosphate of lime is deposited. He distinguishes in the growing bone *four strata*: 1st, the spongy, consisting of bars (Balken) of normal bone structure; 2d, the yellowish spongoid stratum, with groups of large cartilage cells; 3d, the blueish stratum, with large-celled cartilage growth; and 4th, the common cartilage.

"Now, in the normally growing bone an *endogenous* formation of cells takes place in the cartilage stratum; medullary spaces (Mark-Räume), and bone corpuscles are formed; and all these preparatory processes are observed also in the rachitic bone, but without subsequent deposition, or with insufficient deposition, of phosphate of lime, therefore, without ossification of these structures.

"To complete this theory we may safely add the remark, that in bone as in other organic structures a continuous vital movement obtains between the functions of absorption and deposition: so it is obvious that at a high stage of rachitism the already deposited phosphate of lime becomes dissolved and absorbed without an adequate new supply of this crystallizable salt."

After comparing his experience at Pesth with that obtained at Manchester, and with information derived from many medical men practising in this country or in foreign parts, Dr. Merei arrives at the following conclusions:

"From the accounts referred to, corroborated by some others I have received of a more general character, it appears, firstly—that rickets is less frequent in English towns than in many of those of the Continent; and altogether of rare occurrence in Scotland and Ireland: consequently the assumption, that the development of this disease is favoured by northern elevation and damp climate is thus invalidated. From the facts alluded to, however, it cannot be inferred, that in those countries and localities where rickets is seldom observed in its well-marked stages, also those slighter features of the rachitic tendency along with retarded

developmental vegetation which are included in the accounts of Manchester, and which so commonly are overlooked—are of rare occurrence.

“Secondly.—Rickets occurs under a great variety of climates, and the medico-statistical data which we possess at present are insufficient to state what precise kind of climate disposes especially to this disease.

“Thirdly.—Marine atmosphere is highly favorable to infantile development, and consequently to the prevention of rachitism.

“Fourthly.—Rickets, *where it extensively exists*, is everywhere observed in a preponderating proportion among the poor; and it seems to be considerably more frequent among the wealthy on the Continent, than among the corresponding classes in England, with the exception perhaps of a few towns.

“Fifthly.—Improper diet *alone*, under the influence of a pure atmosphere, is insufficient to produce rachitism.

“Sixthly.—Impure air (an over-carbonized atmosphere) of overcrowded and badly ventilated quarters and dwellings, constitutes the most powerful cause of the disorder both in continental countries as well as in England.

“Seventhly.—The influence of unwholesome air is sufficient to cause rickets, in spite of the most wholesome kind of infantile diet: this is proved by the frequency of rickets amongst the educated and wealthy families of continental towns, and to some extent among those of Manchester.

“Eighthly.—Hereditary influence is powerful enough to produce rickets under local and social conditions even the most favorable to infantile development.”

Notwithstanding the swelling of the joints, the curving of the bones, children sometimes look so well and eat so much that the deluded parents cannot reconcile such a state of things with a fearful disease; but our author remarks:

“The careful practitioner, on noticing the phenomena above mentioned, not satisfied with the favorable opinion entertained by the parents, on attentive examination will find: that with the retarded ossification of the fontanelle, and the enlarged wrists and ankles, there is combined therewith some other functional derangement; and he will feel it his duty to draw the attention of the parents to the existing morbid tendency, till then unnoticed. Children presenting a degree, however slight, of that condition of the skull and joints above named, will be found to present the following phenomena in the aggregate, or some of them: avidity for farinaceous articles, bread and potatoes, immoderate eating without due increase of muscular power; the alvine evacuations irregular, in some instances confined, in others greatly abundant and rather liquid, of an unhealthy, variable appearance, sometimes clay-coloured, sometimes dark brown and of a most offensive ammoniacal odour; the urine, in some instances having a slightly brownish tint, and acting strongly upon the blue test-paper, and forming after six or more hours (if not sooner) a sediment, either of a red lithic character, or whitish and mucous; the power of walking of these children, if they are able to walk at all, is neither easy nor firm, and the little strength they possess in proportion



to the bulk of their body, and incapability of adequate muscular exertion, are proofs of their ailment.

"These phenomena—some or all of them—are certainly discoverable in nine out of every ten cases of the slight, often unnoticed rachitic tendency; yet, in the lower ranks I do not think that once in ten such cases are the parents aware of, or pay any attention to, the disorder; and so far as my experience goes, in Manchester as well as in my former position, even among wealthier ranks, physiological and hygienic notions—such, as ought to form part of general education, and to be regarded as one of Lord Ashburton's 'common things'—are as yet so little diffused, that we but too frequently meet with irreparable consequences of the want of knowledge, as regards essential points on the physical management of children.

"Another set of symptoms connected with the above mentioned slight degree of disordered ossification, has direct reference to imperfect hæmotosis, and consists in this: most of these children have a feeble and frequent pulse, and upon auscultation we perceive the chloro-anæmic murmur, particularly on the right side of the neck. These symptoms, however, are less constant than the above-mentioned, and less exclusively characteristic of rachitism."

Pure air, cold sponging, are insisted on as preventives, and with regard to remedial measures, the following passage faithfully asserts upon what remedies we can mostly depend:

"*Internal remedies.*—Remedies calculated to fulfil the chemical indication (to prevent and neutralize acidity) are generally inefficacious in this disorder; and remedies intended to improve one or other of the abdominal functions are seldom of essential use, if not in the long run hurtful. There are instances, of course, in rachitic infants, as well as in others, where an accidental and temporary derangement of these functions will be improved by rhubarb and magnesia, or by minute doses of aloes (from gr.  $\frac{1}{12}$  to  $\frac{1}{8}$  p. d.), or a brisk aperient; but to insist upon the continued use of mercurials to change in these cases the clayish appearance of the stools into a healthier quality, is an indication ill founded in theory, contradicted by practical experience. Such constitutions as rachitic children possess are certainly not to be subjected to the continued action of mercury; and the condition of the blood as peculiar to these habits will be a constant source of disordered abdominal secretions, until it shall have been improved by constitutional remedies.

"The internal remedies—in my opinion the *only* remedies—directly and powerfully beneficial in rickets, are: *cod-liver oil, iron, and quinine.* They have been mentioned already in these pages as being of great value in improving retarded developmental vegetation; in the declared rickets they are imperatively required. The value of *cod-liver oil* is especially acknowledged by credible authorities, among whom Trousseau and Bochart declare it to be the most important remedy in this disease, and almost infallible in its curative effects."

This being Dr. Merei's opinion of the value of cod-liver oil, he has used it extensively in practice, and the result of his comparative trials of the different kinds of this medicine, now before the public, will therefore be duly appreciated.

"Lately I have on several occasions been struck with astonishment on



seeing to what extent commercial men, founding their enterprise upon certificates said to have been obtained from a scientific authority (Dr. de Jongh) and published by several English houses, have succeeded in imposing upon the credulity of the public as regards the pretended superiority of the light brown cod-liver oil above the English. I feel that it would be unpardonable on my part should I hesitate frankly to express my opinion on this subject, founded upon a *comparative* experience with both kinds of oil, both at Pesth and in Manchester, upon a scale most probably larger than most others have had opportunities of enjoying.

"I have ordered the dark and the light brown cod-liver oil to several thousand children affected with different chronic ailments in the children's hospital of Pesth, and to several thousands also in private practice in that town; and during the last four years in Manchester, I have prescribed the English variety to more than two hundred children. The result of my experience is this: that the curative effects are obtained by either kind if it is found to agree with the patient's digestive organs,—but, as was already mentioned, the Norway-oil is less agreeable to the palate and stomach, than the English. And this becomes self-evident upon considering the processes by which they are obtained,—namely, the former being the product of the cod's-liver, after having been left for some time exposed to the air and sun, and thus allowed to undergo a degree of fermentation,—the English (Newfoundland) is directly obtained from the fresh livers of the cod, chopped and enclosed in a bag, and subjected to a vapour bath not exceeding 140° Fht., to avoid any kind of decomposition.

"I have seen it stated, that the *light brown cod-liver oil is four times as active as the English*. Here we may be allowed to ask: what is the active principle of cod-liver oil in the disorders, atrophy, vegetative weakness, and rickets? Nobody has hitherto solved this question satisfactorily. Other oils and fats have not the same effect as cod-liver oil. I feel confident in stating this, from the numerous extensive comparative experiments which I have made with olive-oil, goose-fat, and butter; and to ascribe its curative effects to the minute proportions of iodine contained in it—as some physicians and pharmacologists have done—is a most glaring mistake, since atrophic and rachitic children are never benefited by, and most generally do not bear, even moderate doses of any iodine preparation whatever.

"Judging practically, therefore, after a fair trial of both kinds, I can conscientiously state, that on an average no larger doses are required of the English cod-liver oil than of the light-brown, to produce in the same space of time the same result. From two tea-spoonfuls to a large table-spoonful, in rare cases more (according to age, and the nature or severity of the case) as a daily dose, will commonly after a week or two show some good effects, and improve the rachitic as well as the atrophic disposition. Even if it were true, that the identical curative power is more concentrated in the light-brown than in the English oil, the more palatable and mild qualities of the latter would still decide in its favour.

"In conclusion—cheapness is the only reason why the light-brown oil may be preferred to the other for those patients who are limited in their means, and in case they are able to take and to digest it well."

We hope to meet Dr. Merei again in a more extended field of observation, as his sound judgment and opinions will carry weight when a longer residence in this country will more fully have enabled him to compare the results of his practice in the two very different climates of Pesth and Manchester.

*On Asphyxia Neonatorum.* By Dr. J. HAMILTON, of Falkirk. ('Monthly Journal of Medicine,' May, 1855.)

The experience of Dr. Hamilton affords abundant proof that the lives of a great number of children may be saved by the timely use of the forceps and by persevering efforts at artificial respiration. The proof is that the child was born alive in 416 cases in succession. Some of these children lived for only a short time, from being somewhat premature, from malformation, or from other causes, but they were all born alive. Now, seeing that there is a mortality of 1 in 20, or at most of 1 in 40 in the practice ordinarily pursued, this is certainly a great fact.

The chief peculiarity of Dr. Hamilton's practice is in shortening the second half of the labour process by means of the forceps. The instrument was applied 66 times in 467 cases—not quite 1 in 7—without the loss of a single child. This is a very different practice to that ordinarily pursued, and the result is no less different. Indeed there is reason to believe that this instrument is not used in ordinary practice oftener than once in 200, 300, or even 700 cases, and that the mortality in the cases in which it is used is as high as 1 in 4. Others trust to ergot. Dr. Hamilton rarely ever uses the drug. It is not necessary, however, that we should refer more particularly to this part of Dr. Hamilton's practice, for our readers will find it noticed at length in a former report. ('Abstract,' xvii, p. 312.)

The present paper dwells at some length upon these matters, but its principal object is to direct attention to the fact that many children, apparently dead, may be resuscitated by means of artificial respiration long after the time when the attempt is generally abandoned. Dr. Hamilton bases his remarks upon a case which did not end favorably.

"About ten minutes before 7 p.m., Mrs. C— was delivered of a large male child. A knee presented, and to effect delivery considerable force was required, though the time occupied in passing the head through the pelvis was not very protracted. Even before one leg had passed from the vagina, meconium was voided very copiously, and when the child was born it showed not the slightest movement or trace of sensibility. The usual appliances of rubbing the breast with spirits, plunging the body alternately into cold and warm water, &c., were rapidly had recourse to, but apparently without producing any effect. As a last resource, artificial respiration was employed. In a short time a slight pulsation of the heart became perceptible, and gradually increased in strength, but this was the only motion which could be observed. After having continued the artificial respiration for an hour and a half, a slight tremulous motion of the abdomen was noticed, which at first seemed to be merely

the effect of the heart's pulsation propagated downwards, but which, after another half hour, was distinctly seen to be produced by very weak and rapid respiratory movements. Along with this improvement, the colour of the lips, previously somewhat livid, became of a red, natural hue. In three hours the temperature was good, the pulsation of the heart was strong, and the respiratory motions extended to the upper parts of the chest, and could be continued without the use of inflation for a much longer time than at first. The movements of the heart and chest continued in nearly the same state for another hour, when they began gradually to fail, and they ceased at 11 p.m., after the artificial respiration had been continued for rather more than four hours. No deep inspiration, or other manifestation of sensibility than has been mentioned, was observed during the whole of this period.

"Though the means used in this instance did not save the child, the marked improvement that for a time took place, made me very sanguine at one period that such would have been the case; and, under more favorable conditions, they probably would have been successful. In a good number of other cases, indeed, the same means as were here employed have perfectly succeeded, and in some instances when further perseverance seemed all but hopeless. In the management of the artificial respiration a few circumstances struck me as important, and I shall therefore shortly advert to them, as it becomes a matter of the utmost moment, in such cases as this, that the most effectual means for recalling sensibility should be clearly understood and promptly used.

"For convenience, in inflating the lungs, I had the child laid on the front of the bed, the arms and legs, and as much of the body as possible, being wrapped in warm flannel, which was changed at short intervals, by a person in attendance; under the back part of the neck a pledget of flannel was placed, so as to throw the head gently backwards, and the front of the neck slightly forwards, as I found that the air entered easiest into the lungs in this position. With my left hand I kept steady the head and closed the nares, and with my right I grasped the thorax, so as to be able to compress it—principally in a lateral direction. The air being expelled from the lungs by the right hand, I applied my mouth to that of the child, and inflated the lungs, at the same moment that the elasticity of the ribs was brought into action by the removal of the right hand's pressure. This procedure was regularly repeated, and a certain rhythm, if I may so speak, in the movements, after a short practice, was attained, so that the fatigue, at first considerable, became afterwards greatly lessened.

"As has been mentioned, the heart's action became visible shortly after inflation, in this way, had been begun. In about half an hour, however, the strength of its beat began manifestly to fail, so that I feared it was about to stop altogether. Under these circumstances I tried what effect pressure on the sternum would have, when I immediately found that the action of the heart became decidedly stronger. To satisfy myself that I was not deceived in this, I repeated the process several times with a like result, and, being assured of the fact, during the remainder of the four hours I combined the two movements described, in the following manner:—I first inflated the lungs with my right hand and mouth, three



or four times consecutively, at short intervals, and then tried to imitate the motions of respiration by exerting pressure on the lower third of the sternum, and again quickly removing it, fifteen or twenty times in rapid succession. Whether the pressure on the sternum increased the heart's action by directly stimulating it, or by more thoroughly imitating the respiratory process, or partly in both ways, I do not undertake to say, but the beneficial effect of conjoining the two movements seemed to me manifest. Perhaps, by acting as I did, I, to a certain extent, imitated what the child, had it possessed a greater amount of sensibility, would have done in like circumstances; for we notice, when a new-born child is beginning to recover from a state of asphyxia, 1st, that it makes deep inspirations at considerable intervals; while, 2d, small rapid respiratory movements may be plainly noticed going on during these intervals. The rest, also, which this change in the manipulations allows to the operator, is a matter of some moment, where inflation has to be carried on for any length of time.

"In order to satisfy myself as to the comparative efficiency of these procedures, in introducing air into the lungs, I next day got the permission of the friends to make a few experiments on the body of the child. I first tried the effect of compressing the nares and blowing into the child's mouth, after the chest had been compressed, as previously described. When this was done, even though the muscles of the chest had become rigid, the full inflation of the lungs became obvious, and the sound produced by inflation, as in using a hare's lungs, could be distinctly heard. I then fixed into the trachea a short tin tube attached to a long gutta percha tube, which latter had connected with its other extremity a bent glass tube, partly filled with water. The motion of the water, in this little apparatus, showed, with great delicacy and precision, the effect of movements impressed on the ribs or sternum, and the following are the results obtained: 1st, the largest amount of air forced from, and again taken into, the lungs, was found to be produced by pressing the ribs laterally, as I did before inflating the lungs of this child; 2d, pressure on the lower third of the sternum had a less, though still a decided effect on the air contained in the lungs; and my impression, after making this experiment is, that the much greater number of movements that can, in this way, be given to the chest, make it, *per se*, nearly, if not altogether, as powerful a means as the first for introducing fresh air into the lungs; 3d, forcing upwards the diaphragm, by making pressure on the abdomen, produced on the water in the tube little or no effect.

"I have mentioned that, at the end of three hours, the pulsation of the heart was vigorous, and that the respiratory motions could be noticed to extend to the upper part of the thorax. I then began to use inflation less frequently; and, as the position of the child on the front of the bed was somewhat inconvenient, I thought I might venture to remove it to a table near the fire, on which were placed warm pillows and flannel. Almost immediately upon making this alteration, I noticed that the respiration became less distinct; and in a short time the change was so decided as to compel me to replace the child in its former position; after which, both the heart and chest began shortly to move nearly as before. Perhaps the cause of this unfavorable change may have been that, in the

new position, the air did not get access to the lungs so easily as formerly, although I did not observe any very decided difference in this respect; but there was also another circumstance which I suspect might exercise some influence in producing the unfavorable change. While the child lay on the bed, the head and upper part of the body occupied a slightly dependent position; whereas, on the table, the whole body was placed horizontally. Might it not be, that, in the former position, the arterialized blood found its way more easily to the nervous centres, and thus preserved a greater amount of sensibility? Whatever may have been the cause, the fact referred to shows how slight are the alterations, under such circumstances, which may produce important differences in the result. In this respect, I recollect, many years ago, while assisting Dr. G. C. Holland in performing experiments on artificial respiration in rabbits, in connection with his work on 'Animal and Organic Life,' being struck with the fact, that even such a slight circumstance as occasionally altering the position of the animal, had some effect on the experiment. If, for example, while the lungs were being inflated, the action of the heart became weak, the posterior extremities had merely to be elevated to cause the heart to be immediately, for a time, stimulated into vigorous action. At the commencement of operations, in the case I have been relating, I kept this observation in mind, and perhaps the alterations of position which were at that time frequently made, assisted in keeping up the then faint action of the heart.

"It is hardly necessary to say that, before beginning the inflation of the lungs, I introduced my finger into the pharynx for the purpose of clearing away any mucus which might be lodging at the top of the wind-pipe; and also, that the inflation itself was performed very gently, in order to avoid the risk of rupturing the air-tubes. I may remark, however, that the introduction of the finger into the pharynx may have some effect in rousing the dormant sensibility in such cases, as we are by this means acting upon parts peculiarly susceptible of being powerfully stimulated. To increase this stimulus, and also in the hope that a small portion might find its way into the stomach, or be absorbed, I occasionally, during the continuance of inflation, dipped my finger into spirits, and rapidly passed it down to the top of the œsophagus."

#### IV.

#### REPORT ON PHYSIOLOGY.

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1. *On the Extensive Diffusion and Frequency of Starch-Corpuscles in the Tissues of the Human Body.* By THOMAS ALBERT CARTER. ('Edinburgh Medical Journal,' August, 1855.)
2. *Corpora Amyloidea in the Human Lens.* By Mr. R. TAYLOR. ('Pathological Transactions,' vol. vi.)

THE facts which are here presented are of great interest, as illustrating the intimate connexion which exists between the animal and vegetable world by showing that starch is a proper constituent of animal bodies. We have already referred to this important point ('Abstract,' xix, 365), and this reference must suffice.

In his paper, Mr. Carter tells us why the presence of starch-granules so long escaped the notice of microscopical observers; and his explanation is no doubt the correct one. It is this: It is well known to microscopists that fatty matters, frequently in the form of oil-globules, are almost invariably interspersed among the morphological elements of which the various organs and tissues are composed. Now to these globules the animal starch-corpuscles bear a very marked resemblance, not merely in general form, but likewise in refractile properties. Can it then be looked upon as a matter of surprise that the two have been confounded by those who have neither by chemical nor other means endeavoured to discover their nature? And in some instances when recognised, as they often have been in the urine, they have been considered either as fraudulent additions to, or accidental admixtures with, this excretion; possibly with correctness in some instances, though by no means in all.

Mr. Carter also states that he has found starch-granules in the cerebrum, cerebellum, liver, lung, spleen, kidney, in the striped muscle and areolar tissue of the sheep; and in the liver, lung, and ligamentum nuchæ of the ox. So that their presence is by no means peculiar to man. He has also found them in healthy blood drawn from the finger, in pus, cancer-juice, chronic lymph, bronchitic sputum, ichthyosis scabs, tubercular matter from the lung, and in the urine of persons suffering from Bright's disease, dyspepsia, acute articular rheumatism, pneumonia, &c.

In these inquiries two distinct varieties of starch-corpuscles were met with—the one (described by Busk) resembling wheat-starch; the other, and rarer kind, corresponding in every particular with that



derived from the potato. In dimensions they vary from the  $\frac{1}{125}$ th to the  $\frac{1}{4000}$ th of an inch, the usual size being the  $\frac{1}{1000}$ th.

The following are the facts; they were taken without any selection, and they are given in the order in which they were observed:

*“Observation 1.* A man, N., æt. 39 years, who died of phthisis pulmonalis, complicated with albuminuria.

“A micro-chemical examination of the liver, which was of twice its ordinary weight, and presented in a marked degree the characters of waxy degeneration, revealed the presence of starch in great abundance. One demonstration exhibited fifty-two bodies of the ordinary size, adhering to, or imbedded in, a mass of transparent homogeneous tissue.

“In the spleen and kidney, which were also ‘waxy,’ starch was found, abundantly in the former, in the latter somewhat sparingly.

*“Obs. 2.* A woman, J. B., æt. 59 years, who died of apoplexy.

“The tubular matter of the cerebrum, immediately surrounding the apoplectic clot, was subjected to an examination, and in it were found, associated with numerous compound granular cells, numbers of starch corpuscles. This observation does not apply to the healthy portions of the same brain, for, after a prolonged search, none could be detected. An ordinary amount was observed in the spleen, kidney, and liver of this subject, all of which appeared perfectly normal.

*“Obs. 3.* A female, M. R., æt. 24 years, who died of phthisis pulmonalis.

“In the liver, which was somewhat waxy and slightly enlarged, were found starch bodies of irregular form, as if developed in masses under pressure.

“The spleen yielded a few. This, like the former organ, was in some degree waxy.

“They were very numerous, and of large dimensions, in the kidney, many attaining to the  $\frac{1}{400}$ th part of an inch in diameter.

“I noticed, in the pancreas and mesenteric glands of this subject, numbers of corpuscles, generally of oval form, measuring about the  $\frac{1}{400}$ th of an inch in their longest, and about half that in their shortest diameter. The hilum was placed near one extremity, with a series of concentric ellipsoidal markings surrounding it. Moreover, when seen by polarized light, they exhibited the four black or coloured radii diverging from the hilum; in fact, they were in every respect identical with the amyllum of the potato.

*“Obs. 4.* A female, J. H., æt. 23 years, who died of phthisis pulmonalis.

“Starch was found in the liver, kidney, spleen, pancreas, mesenteric glands, and supra-renal capsules, all of which organs were healthy, with the exception of the mesenteric glands, these being slightly enlarged: also in an indurated tubercular mass from the lung.

*“Obs. 5.* A man, J. B., æt. 38 years. The immediate cause of death unknown. The heart and larger vessels, however, were diseased.

“In the liver (slightly enlarged, but in other respects healthy), kidneys, supra-renal capsules, spleen, corpora striata, pituitary body, and Pacchionian glands, starch was found. None could be discovered in the pancreas, after a very careful and prolonged search. This organ was very flaccid, and apparently diseased.

*“Obs. 6.* A man, Y., æt. 17 years, who died of general anasarca.

“The liver of this subject was congested, and to a small extent fatty; the spleen rather pulpy; both exhibited the usual amount. The pancreas, which was flabby, yielded a very small quantity. In a mass of degenerate pus found adhering to the peritoneal surface of the bladder, and in chronic pericardial exudation matter, they were also present.

" *Obs.* 7. A boy, P., æt. 14 years, who died of apoplexy.

" The liver, spleen, kidneys, super-renal capsules, thyroid body, tonsils, and brain, contained starch in greater or less quantity. The kidney, in this case, was the viscus in which they were principally developed, both as regards number and size. By mensuration, I ascertained that one of them exceeded the 1-200th of an inch linear. All the organs appeared quite healthy.

" *Obs.* 8. A youth, æt. 17, whose death was occasioned by an enormous fibro-nuclear tumour growing from the abdominal parietes, and general anasarca.

" The existence of starch was determined in the liver, spleen, supra-renal capsules, and pancreas; in the last-mentioned organ the rarer variety. It was found in all parts of the tumour with the greatest facility.

" *Obs.* 9 and 10. A female, E. G., æt. 16 years, and a man, A. C., 43 years of age, both of whom died of phthisis.

" The following organs, which appeared healthy in both, contained the ordinary quantity: the livers, kidneys, pancreatic and mesenteric glands. The supra-renal capsules of A. C. were examined, and with a like result.

" *Obs.* 11. A man, æt. 33 years, who died of jaundice, together with œdema and erysipelas of the lower extremities.

" All the organs mentioned in the two preceding cases were examined, and positive evidence of the presence of starch obtained; but the quantity in each was excessively small, more especially in the pancreas. With the exception of the liver, which was in an incipient state of cirrhosis, and deeply impregnated with the colouring matter of the bile, all the viscera were normal.

" Among, and also within, the wavy bundles of fibrous tissue from the mesentery, starch grains of the ordinary dimensions were scattered with tolerable regularity, but for the most part at rather wide intervals.

" *Obs.* 12. A child three weeks old. Cause of death unknown.

" The liver contained a more than ordinary quantity; the kidney, spleen, and pancreas, the usual amount.

" *Obs.* 13. A man, D. L., æt. 25 years, who died of diabetes mellitus.

" Starch was of excessively rare occurrence in the liver of this subject; less rare in the pancreas; and the general amount observed in the spleen, kidneys, mesentery, and the white and grey matters of the cerebrum. Little was seen in the cerebellum."

Mr. Taylor's case reveals the presence of starch-granules in the human lens. It is as follows:

" The patient, a woman upwards of 70 years of age, came under my care at the Central London Ophthalmic Hospital, for inflammation of the eyeball, of a peculiar and unusual character, which commenced in the cornea, and thence extended backwards to the deeper-seated tissues. Through the small portion of the cornea that retained its transparency, the anterior chamber was seen to be filled with a body of a yellowish-white colour, which almost completely concealed the iris.

" As there was no hope of the restoration of vision; as the woman suffered constant severe pain, which had seriously impaired her health; and as the other eye was beginning to suffer sympathetically, I removed the cornea, and permitted the escape of the crystalline lens, with a small quantity of the vitreous humour. This operation gave complete and permanent relief.

" It was found that the anterior chamber was completely filled by a cake of lymph, moulded to the form of the cavity, and incorporated with the iris, little trace of which remained, except pigment.

" The lens was examined by Dr. Kirk, who has favoured me with the following report:—"The parts examined consisted of the lens with its cap-

sule. The anterior capsule had been considerably lacerated; the posterior capsule was continuous and undisturbed. The structure of each was normal. The lens was hazy, and of a deep amber colour. The surface was soft, and the superficial lens-fibres were normal. The nucleus was rather dense; its fibres very unequal in size, much atrophied, and granular in appearance.

“Immediately under the capsule, and imbedded in the superficial lens substance, was found a layer of peculiar bodies, which varied much in form and size. The predominant form was the spheroidal, more or less elongated; sometimes larger at one extremity than at the other. Many were united together linearly, as though they had sprouted from each other; several had linear prolongations; others were of most eccentric forms. In a few, a series of concentric markings was evident; but in most they were absent. Under the polarizer, several of the larger bodies presented a distinct cross; in the smaller ones the cross was faintly marked, or altogether absent. Tested with tincture of iodine, they assumed a deep blue colour, gradually increasing in intensity till they became opaque.

“In making the examination, every precaution was taken. Distilled water was used, and the covering glasses were carefully cleansed. The bodies were first observed on the anterior surface, where the capsule was torn, and where it was possible that foreign matter might have found entrance; but doubt was removed on finding them in still greater abundance under the posterior capsule, and imbedded in the lens substance.’”

*Muscular Contraction, a Physical Phenomenon; a sketch of the argument, with alterations and additions.* By CHARLES BLAND RADCLIFFE, M.D., L.R.C.P., Assistant-Physician to the Westminster Hospital, &c. (*‘Medical Times and Gazette,’* 16th, 23d, and 30th of June, 1855.)

In the present article it is proposed to give a summary of the principal arguments which seem to show the necessity of a complete revolution in the opinions respecting muscular contraction. This summary is divided into four parts. In the first part, it is proposed to show that muscle is not stimulated to contract by any of the commonly reputed stimuli,—electricity, nervous influence, blood, light, heat, and the rest. In the second part, it is proposed to examine into the nature of muscular contraction, and point out reasons for supposing that this contraction is nothing more than a passive physical consequence of the molecular attraction of the muscle. In the third part, it is proposed to consider the special muscular movements which are manifested in the coats of vessels, and to show, not only that these movements can be explained on no other law than that which has been stated, but that the law gives the clue to the interpretation of “capillary motion” and the rhythm of the heart. In the fourth part, it is proposed to glance at the pathology of muscular contraction, and show that this is in harmony with the physiological premises.

I. In this part, the object is to show that muscle is not stimulated to contract by any of the commonly reputed stimuli,—electricity, nervous influence, blood, heat, and the rest.

1. *Of Muscular Contraction in relation to Electricity.*—Living muscle is the seat of electrical currents which proceed from the



ends to the sides of the fibres, and which may be traced, not only in the entire muscle, but in the individual fibres. These currents continue during life, and are not finally extinguished until the occurrence of *rigor mortis*. They are designated under the general name of "muscular current," and they will be so designated here.

The muscular current continues during life, but it does not always continue at the same pitch of intensity. It changes during muscular contraction. Matteucci, who first directed attention to this subject, thought it became stronger at this time. Du Bois-Reymond ('*Untersuchungen über Thierische Electricität*,' Berlin, 1848), on the contrary, first thought it became weaker, and then doubted his original conclusion. The facts, however, are sufficiently simple, and his conclusion need not have been doubtful.

If a living muscle be connected in a proper manner with the galvanometer, the needle immediately reveals the muscular current by diverging to a certain extent from zero; and this it does until contraction is induced, when it immediately moves to the other side of zero. The needle, that is to say, is acted upon by a *reverse* current during contraction. There is no doubt about this, but there is doubt about the origin of the current. Is it in the muscle or in the instrument, for it may be in either or both? This question is answered by testing separately the condition of the muscular current during rest and during contraction (by breaking the connection and depolarizing the galvanometer between the two experiments); and the answer is, that the needle moves in the *same* direction during contraction as it moved during rest, but not to the same distance from zero. In other words, the muscular current is *weakened during contraction*, but not changed; and hence it follows that the backward movement of the needle during contraction in the former experiment was due to the secondary reverse current which traverses the coil of the galvanometer upon the diminution of the current which had previously passed from the muscle through the coil.

Judging from the needle of the galvanometer, then, the muscular current appears to be *weakened during contraction*, and why then does Du Bois-Reymond doubt a conclusion which appears to be so obvious. He doubts, because the phenomenon of "secondary contraction" (which is induced in a second muscle by placing its nerve in a certain manner upon the muscle which is being made the subject of experiment) appears to reveal the presence of certain *changes* in the muscular current of the primarily contracting muscle which are not revealed by the needle of the galvanometer. Now, certainly, the "secondary contractions" are due to certain *changes* in the muscular current of the primarily contracting muscle. All are agreed about this. But what is the nature of the change? Du Bois-Reymond argues that the "secondary contractions" could not arise if the muscular current (which causes them) remained *constant*, and he therefore concludes that there must be certain *oscillations* in the current during contraction, although the sluggish needle of the galvanometer gives no evidence of them. But this is begging the whole question, as will be seen presently.

The commonly received opinion respecting the action of artificial

electricity upon muscular contraction is that there is no contraction so long as the current is constant, and that contraction immediately follows whenever the current rises or falls in any manner. Is it so? What are the facts, and is this their significance?

When an artificial current is passed through the leg of a frog in a direction opposed to that of the muscular current of the limb, there is a *strong* contraction on making, and a *slight* contraction on breaking, the circuit, and there is *no* contraction during the passage of the current. In other words, there is a *strong* contraction at the moment when the artificial current neutralizes and is neutralized by the muscular current; there is *no* contraction when the artificial current prevails and is passing through the muscle; and there is a *slight* contraction when the artificial current is suspended. This latter contraction is less marked than the other, and this may well be, for the slight reverse current, which must be set up in the muscle on the suspension of the primary artificial current, passes in the same direction as the muscular current; and because this reverse current is too feeble to interrupt the re-establishment of the muscular current, the muscular current resumes its action with such rapidity that there is no time for any marked degree of contraction. In a word, the simple fact appears to be that contraction is absent when electricity is manifestly present, and present when electricity is in all probability absent.

When, on the other hand, the artificial current is passed through the leg of a frog in the same direction as the muscular current, there is a *slight* contraction on making, and a *strong* contraction on breaking, the circuit, and there is *no* contraction during the passage of the current. There is still *no* contraction while the muscle is acted upon by the full force of the artificial current; and there is a *strong* contraction when the artificial current is suspended, and when the reverse current which is set up on this suspension must, for the time (the two currents being contrary), neutralize and be neutralized by the muscular current, which muscular current recovers itself when the artificial current is suspended. But what of the *slight* contraction which occurs on making the circuit,—when, that is to say, the artificial and natural currents coincide, and when, apparently, they ought to intensify each other? This is, indeed, a difficulty, but the explanation appears to be that the two currents do not intensify each other, although they coincide in their direction; but that the artificial current, being the stronger, suspends the muscular current, by altering for the time that definite molecular arrangement upon which the muscular current depends. In this case, therefore, the contraction is coincident with the moment of suspension, the contraction being slight because the artificial current establishes itself without having to encounter any direct opposition from the muscular current. In this experiment, then, as in the other, there is no sufficient reason for referring the contraction of the muscle to the stimulus of electricity, and the rule still appears to be that contraction is absent when electricity is present, and present when electricity is absent.

It is the same also if the artificial current be passed across the muscular current, for the contraction still happens on making and break-

ing the circuit, and not during the passage of the current. There is *no* contraction, that is to say, while the muscle is acted upon by the artificial current; there is contraction before the artificial current is established, and at the moment when this current is conflicting with the muscular current; and there is contraction before the muscular current is re-established, and at the moment when the returning muscular current is conflicting with the reverse artificial current.

The fact, however, which is more conclusive than any other as to the influence of artificial electricity upon muscular contraction has just been furnished by M. Eckardt ('Grundzüge der Physiologie des Nervensystems,' Giessen, 1854). *It is that a tetanized muscle is relaxed by the passage of a constant current of electricity.* This fact is, indeed, an *experimentum crucis*, when taken in connection with the facts already cited. These have shown that contraction is absent when electricity is present in the muscle; this shows that contraction must cease if electricity be imparted.

It would thus appear that the opinions generally held respecting the action of electricity upon muscle are not tenable, and that it is not enough to say that there is no contraction so long as the current is constant, and that contraction immediately follows whenever the current rises or falls in any manner. On the contrary, the simple facts appear to be that contraction is absent when the natural or artificial currents are present, and that contraction is only present when the muscular current is in some degree absent. Nay, so antagonistic is the current to contraction, that it even dispels it when present. And if this be the case, then the conclusion which Du Bois-Reymond has drawn from the phenomenon of "secondary contraction" falls to the ground, for all that is necessary to the production of this phenomenon is, not that there should be *oscillation* in the muscular current of the primarily contracting muscle, but simply that this current should become *weakened*, so that the position of the needle of the galvanometer and the phenomenon of "secondary contraction" alike point to the same fact, and that is a *fall in the muscular current during contraction*.

—In no point of view, then, does muscle appear to be stimulated into contraction by electricity. On the contrary, the only safe conclusion which can be drawn from this intricate evidence is altogether opposed to this idea. It is that muscular contraction is antagonized by electricity.

2. *Of Muscular Contraction in relation to Nervous Influence.*—Comparing involuntary with voluntary muscles, the involuntary muscles are found to be most disposed to contract. They contract less readily and energetically, but when they do contract the contraction is more abiding. But these very involuntary muscles are the muscles which are less liberally supplied with nerves, and hence the disposition to contract appears to be inversely related to the supply of nervous influence. At any rate this was the inference which Hunter drew from the fact; for he says, "the voluntary and involuntary muscles, having their quantity of motion in an inverted proportion to their quantity of nerves, is a strong argument against the nerves being the cause



of muscular motion." ('Hunter's Works,' by Palmer, vol. iv, p. 213.)

Nor is this inference unsupported by other facts.

The first of these is furnished by M. Eckardt in an experiment in which he tests the influence of heat upon the "irritability" of the nerve. In this experiment, the prepared leg of a frog, with a large portion of its nerve attached, is immersed in water at various degrees of temperature. At the natural heat of the animal—about 30° Reaumur—the "irritability" of the nerve is not appreciably affected, for, on touching the nerve with a needle, the muscle contracts as readily as it did before it was put into the water. As the temperature rises, however, the "irritability" progressively diminishes; and when the thermometer stands at 54° Reaumur, or thereabouts, it is no longer possible to provoke contraction by touching the nerve. The "irritability" of the nerve is lost, and then it is that the muscle is made to contract by the heat. Now the point of interest to the question at issue is this, that the muscle contracts when the temperature is sufficiently high to destroy the "irritability" of the nerve. In other words, the muscle is exhibited as contracting coincidentally with the loss of "irritability" in the nerve; or, as M. Eckardt expresses it, "das Zustandekommen der Zuckung durch eine momentane Zerstörung des Nerven bedingt sei." ('Grundzüge der Physiologie des Nervensystems,' Giessen, 1854.)

A fact of similar significancy is to be found in the changes which Du Bois-Reymond has shown to take place in the electrical currents of nerves, for these currents (which may be designated as the *nerve current* hereafter) are found to become weakened during contraction. They behave precisely like the muscular current (op. cit.) Is this, then, an argument that the nervous influence is similarly diminished at this time?

A third fact is one which seems to show that the nervous centres induce contraction by suspending the nerve current. The fact is this. If a frog be thrown into a state of tetanus, its nerve currents are found to be weakened wherever they are examined; but if a nerve be divided, the tetanus immediately ceases, and the nerve current returns in the parts below the section. What then? Is it not the inference that the nervous centre had induced the tetanus by suspending the nerve current? and, if so, is there not a collateral inference that nervous influence had been suspended at the same time?

But what is nervous influence? Is it a distinct agency, or is it (so far as the muscles are concerned) the influence exercised by the nervous centres in and through the electrical currents of which nerves and muscles are undoubtedly the seat? Is it that living *quiescent* centres, nerves, and muscles, are all the seat of these currents, and that muscular contraction is brought about by the suspension of these currents? Is it that *plus* becoming *minus* in the nervous centre (be this by an act of the will or by other means), a similar change from *plus* into *minus* takes place by *conduction*, first in the nerve and then in the muscle, and that contraction is the effect of this change in the latter organ. According to this view nervous influence is presented to the mind rather as a process than as an agency, and the

two facts last mentioned become direct instead of indirect illustrations of what takes place in the nerves during muscular contraction. Be this as it may, however, and be the nervous influence an agency or a process, the presumption which arises out of the facts already mentioned is, that the influence in muscular contraction is one of e-nervation and not one of in-nervation.

But if muscular contraction holds this relation to nervous influence, ought not the muscles to be contracted when the muscle is cut off from the nervous centre by dividing or otherwise paralysing the nerve, or when the action of the nervous centre is diminished, as during sleep? Is not the fact that the muscles are generally relaxed under these circumstances a proof that the nervous centres communicate something to the muscles which is necessary to contraction? By no means.

In explaining these apparent objections it is only necessary to remember the law of the nerve current and muscular current.

It is necessary to remember that the nerve current is always present in living nerve during quiescence; that this current is weakened when the nerve is occupied in causing contraction; and that it is restored to its former degree of power when the nerve has ceased to cause contraction.

It is necessary to remember that the muscular current is always present in living muscle during quiescence; that this current is weakened during contraction; and that it is restored to its former activity immediately after contraction.

It is also necessary to remember that these particular currents are inherent in nerve and muscle, and that they continue to be manifested in detached fragments of nerve and muscle for some time after they are removed from the body. They are, no doubt, weakened under these circumstances, but their presence is unmistakeable.

These are the fundamental facts which afford the clue to the solution of the difficulties alluded to, and of many others which remain behind. It does not follow, then, that a muscle ought to remain contracted when it is cut off from the nervous centre by dividing or otherwise paralysing the nerve. On the contrary, there are no natural means of suspending the muscular current now that the nerve is paralysed; and, according to the premises, therefore, the muscular current, which is always present when the muscle is left to itself, will always keep the muscle in the relaxed state. This muscle may, and generally does, contract at the instant of paralysis, because its current may then be weakened by being cut off from the currents which play in the nerves, and, probably, also in the nervous centres. It may contract momentarily under the touch of a foreign body, as will be explained presently. It may contract permanently when the paralysis has continued some time, and when the polar and nutritive action of the muscles have both failed, as in those long standing cases of paralysis which are so well described by Dr. Todd. It will contract permanently in *rigor mortis*, when the muscular current has ceased altogether. But a healthy living muscle does not remain contracted when the nerve is divided or paralysed in any other way, and it ought not to remain contracted because the muscular current con-

tinues, and because there are no *natural* means of suspending this current now that the muscle is cut off from the nervous centres.

And if the muscles ought not to remain contracted, in cases where the influence of the nervous centres is cut off by paralysis of the nerve, they ought not to remain contracted in sleep, where the influence of these centres is only diminished. At the moment of falling asleep, there are usually, if not invariably, some muscular startings, which may show that some nervous influence has been cut off from the muscles; but these startings once over, the nervous centres, the nerves, and the muscles resume their polar play; and, though their action is probably less energetic than when the nervous centres were in the waking state, it may be assumed to be more than sufficient to counteract any very appreciable degree of muscular contraction, seeing that the action which continues in a muscle for some time after its removal from the body, is sufficient to do this. Muscular contraction may indeed occur during sleep, but if it does do so, there has been some additional suspension of the action of the brain, or other nervous centre, either through the influence of the will in a dream, or by some unusual failure of the circulation, such as generally operates when convulsion or spasm is brought about during sleep. But there is no reason whatever for supposing that the muscles should remain contracted during sleep.

On reviewing the whole evidence, therefore, there does not appear to be any good reason for believing that muscle is stimulated to contract by nervous influence, and there is much evidence to the contrary.

3. *Of Muscular Contraction in relation to the Blood.*—It is equally difficult to suppose that muscular contraction is in any way caused by the blood. The tendency to prolonged contraction appears to be inversely related to the supply of blood; thus this tendency is greater in the voluntary muscles of fishes and reptiles than of mammals and birds, greater in involuntary than in voluntary muscles, and greater in the muscles of any given animal during the state of hybernation than during the period of summer life. The fact, also, that the state of *rigor mortis* may be relaxed more than once, and the lost “irritability” restored to the muscle, by the injection of living blood into the vessels,—a fact which has been recently and repeatedly verified by M. Brown-Séguard,—appears to be in direct contradiction to the idea that muscular contraction is stimulated by the blood.

Nor is it necessary to have recourse to the contradictory doctrine, that “the degree of irritability is inversely related to the rate of respiration,” in order to account for the first-named of these phenomena. On the contrary, it is only necessary to suppose that the force of the muscular current is in direct relation to the supply of blood, and that the contraction is resolved, by the re-establishment of this current, with a rapidity which bears a direct relation to the supply of blood; and then the more marked disposition to contract, when the supply of arterial blood is deficient, means nothing more than that the muscular current, and the attendant relaxation of the fibre, are re-established with greater slowness.



There are, however, sundry facts which seem opposed to the idea that muscular action is antagonized by the blood.

In hemorrhage, an animal is convulsed when its state verges upon syncope, and the convulsion seems to depend upon loss of blood; but, when its state is one of actual syncope, the convulsion passes off, and the muscles remain relaxed until the occurrence of *rigor mortis*. It seems as if the convulsion requires the stimulus of a certain amount of blood.

In asphyxia there is a similar order of phenomena. In this state the involuntary muscles are first affected, and the intestines writhe about like so many snakes; then the convulsions become general; but when the blood has entirely lost its arterial properties, and the asphyxia is complete, the convulsions cease, and the muscles are perfectly relaxed. It still seems as if the stimulus of the aerated blood is necessary to the convulsion.

In death, also, the convulsion of the agony ceases when death gains the mastery, and the muscles remain relaxed until the occurrence of *rigor mortis*; and so it might be expected, for, as far as the circulation is concerned, death is only syncope or asphyxia, in which there is no rallying.

In all these cases, however, there is a fallacy, and in reality they afford no manner of countenance to the idea that muscular contraction is stimulated by the blood. The facts remain, but not the interpretation which has been put upon them.

When the muscles cease to be convulsed in syncope, asphyxia, or death, this cessation is certainly not due to loss of contractile power in the muscles, for these very muscles contract vigorously under the influence of galvanism, or when pricked with a needle, and in the end they contract firmly and entirely in *rigor mortis*. What is lost is the faculty of responding to certain changes in the nervous centres. When the convulsion ceases, the simple fact appears to be, that the nerves have ceased to be conductors, and that the change from *plus* into *minus* in the action of the nervous centres (which is caused by hemorrhage on the one hand, and want of arterial blood on the other), is no longer *conducted* along the nerves to the muscles; and being no longer conducted, the muscular current is left at liberty to resume its play, and relax the muscular fibre. This interpretation is allowable, for the muscular current is far less dependent upon the supply of blood than the nerve current. Now, there is good reason for supposing that the nerves have ceased to be conductors in syncope, asphyxia, and death. When the circulation in the hand is suspended by immersion in iced water, the sense of touch and the power of movement are partially or wholly destroyed. When the principal vessel of a limb be tied, a similar result ensues, until the collateral circulation is established. In each case, also, the power of provoking reflex movements is diminished or destroyed. On the other hand, the sensibility, and the command over movement, are both increased when the circulation is roused by warmth, or in any other way. Facts such as these serve to show that the nerves require a certain supply of blood to enable them to act as conductors, and they warrant the conclusion that the nerves must have ceased to be conductors at the time when

the convulsions of syncope, asphyxia, or death come to a termination, for at this time the supply of blood to the nerves is less than it is in the experiments in which the hand is plunged in iced water, or in which the principal vessel of a limb is tied,—a conclusion which is collaterally supported by the fact, that the nerves, under these circumstances, have ceased to convey sensory and volitional impressions. And if so, then it is every whit as intelligible that the convulsions should cease, under these circumstances, as that a tetanized muscle should relax when the nerve is divided.

According to these premises, it is quite intelligible that convulsions should be the consequence of a state of circulation verging on syncope, and that convulsion should cease in syncope. In the state verging on syncope, the amount of blood passing through the vessels is insufficient to keep up the proper action of the nervous centres, but it is still sufficient to keep up some degree of conducting power in the nerves, and hence the convulsions; for, the nerves being conductors, that failure in the action of the nervous centres which is dependent upon the want of blood, conducted along the nerves, entails a corresponding failure in the muscular current, of which contraction or convulsion is the result. In actual syncope, on the other hand, the circulation is no longer sufficiently active to preserve the conducting powers of the nerves, and hence the cessation of the convulsions; for, the nerves being no longer conductors, the failure in the action of the nervous centres, however absolute, no longer involves a corresponding failure in the muscular current, and, not involving this, the current returns, and the convulsions are at an end.

The same train of reasoning applies to the case of asphyxia. So long as the blood is sufficiently aerated to preserve the conducting powers of the nerves, any failure in the action of the nervous centres, which is itself brought about by the want of arterial blood, may issue in convulsion; but when the nerves cease to be conductors—as they do when the blood has lost its arterial properties—then the muscular currents are no longer suspended by the suspension of the action of the nervous centre, however complete this suspension may be; and not being suspended, the muscular current resumes its sway, and the convulsion is done away with.

For the same reasons, the tremors, or convulsions, or cramps of the agony cannot continue after death; for if the nerves cease to be conductors in syncope and asphyxia, they must cease to be conductors when all circulation is at an end, and the blood stagnant.

According to the premises, therefore, it is quite intelligible that convulsion should appear in a state *tending to* syncope, asphyxia, and death, and yet cease *in* syncope, asphyxia, and death, and this without supposing for one moment that the immediate stimulus of blood is necessary to muscular contraction. Indeed, there is no one fact which can serve to show that muscle is stimulated to contract by the blood.

4. *Muscular Contraction in relation to various Mechanical Agents.*—Nor is it by any means probable that muscle is stimulated to contract by mechanical agents. Instead of exciting the bladder to contract, the urine accumulates, the viscus expands, and contraction seems to

*happen* when further expansion is productive of uneasiness or pain. Instead of exciting the uterus to contract, the germ grows and the womb enlarges proportionately, and contraction, to all appearance, does not happen until the growth of the fœtus is perfected, and the stimulus of that growth at an end. For nine long months the fœtus seems to have excited the uterus to continual expansion, and, to say the least, it is not easy to imagine how it can excite contraction at the time of labour. Arguing from the history of pregnancy, the probabilities, as measured by time, are those of nine months to as many hours against such a view. Discarding theory, indeed, the simple fact appears to be that the fœtus grows and causes the uterus to expand by the stimulus of its growing presence, and that it does this until that growth begins to trench upon the supplies which are necessary for the proper nourishment of the mother. Then the child becomes a source of exhaustion to the parent, and this exhaustion, reacting upon the uterus, brings back the state of contraction,—for, if the uterus expanded in consequence of stimulation, it must return to the state of contraction if the degree of stimulation be diminished, and this equally whether this diminution be caused by the death of the child, or by the child having lived so long that it begins to starve the mother by its too clamorous wants. In either case contraction must happen if the uterus had previously been kept in a state of expansion by stimulation. This contraction compresses the placental vessels, and depresses the life of the fœtus by interfering with the proper aëration of the fœtal blood; and this depression, reacting upon the uterus, is attended by a further degree of contraction. This contraction, like the first, compresses the placental vessels, and depresses the life of the fœtus by interfering with its respiration; and this depression, extending to the uterus, necessitates a corresponding degree of contraction. Again and again, contraction leads to contraction by the same process, and in this way the uterus acts upon the fœtus, and the fœtus reacts upon the uterus, with ever-increasing contraction as the result, until the completion of birth. At all events, it is impossible, upon any rational view of parturition, to refer the contraction of the uterus to any *stimulation* on the part of the fœtus, without ignoring the whole previous history of pregnancy. Nor can it be successfully objected to this view that the bladder is excited to contract by a stone, or the uterus by the clots of blood which occasionally remain after delivery. There is no evidence whatever that the stone acts in this manner. The bladder is morbidly sensitive under these circumstances, and a very small quantity of urine is enough to cause distress or pain, and thus the *will* or *instincts* are roused to empty the bladder more frequently than usual. The uterus, also, goes on contracting after delivery, until the process be complete, and this equally whether there be clots in the cavity or not. If there are clots in the cavity, it only shows that more of the process of contraction remains to be effected than there ought to be; but it cannot show that the clots excite contraction, for in other cases the same contractions take place, and more effectually where no such clots are present.

It is not even certain that a needle stimulates contraction. The



muscle does not always contract under these circumstances, and when it does, there is some reason to believe that the contraction may be due to the discharge of electricity previously present in the muscle. The fact that there is a disappearance of electricity at this time, and the known analogy between the structure of the muscle and of the electrical organ of the torpedo, and between the circumstances attending the production of contraction on the one hand and of discharge on the other, are, to say the least, in favour of this supposition. But it may be objected that this contraction is provoked by the touch of a piece of glass or of any other non-conductor; and this objection is not easily disposed of. It may be, however, that that molecular arrangement of the muscle which is necessary to the existence of the muscular current is broken by the *pressure* of the touching body, in which case there would be a loss of action similar to that which would happen in a galvanic pile, if the pile were broken by pressing asunder the plates at any point. Or it may be that the polar condition of the muscular molecules is so delicately balanced as to be disturbed and, for the time, diminished, by the simple attraction which operates between the sealing-wax or glass *as matter* and the muscular molecules *as matter*.

Under any circumstances, however, there is very insufficient reason for supposing that muscle is stimulated to contract by any kind of *mechanical* agent.

5. *Muscular Contraction in relation to Heat.*—The effects of temperature upon muscular contraction appear at first sight to be at variance with the premises, but they are not so in reality. The plain facts are that muscle will bear considerable variations of temperature without contracting, and that it is thrown into a state of marked contraction, and that equally, by a very high or a very low temperature.

The explanation of this apparent paradox is to be found, not in the immediate effects of the temperature, but in the changes which are wrought by the temperature in the electrical condition of the muscle. So regarding the phenomena, it is quite intelligible that contraction should be caused by low degrees of temperature, for M. Matteucci has shown that the muscular current is suspended under these circumstances. On the other hand, there is every reason to believe that this current is similarly suspended by heat when heat causes contraction. This is certainly the case with regard to the “irritability” of the nerve, as is shown by M. Eckardt in the experiment already cited; and it can scarcely be otherwise with regard to the polar action of both nerve and muscle, for the experiments of M. Du Bois-Reymond go to prove that this action is diminished in every form of muscular contraction. It follows, also, from the same experiments, that the muscular current is not depressed to the point of allowing contraction by any intermediate degrees of temperature; and hence, upon the same premises, it is quite intelligible that the muscle should bear all intermediate degrees of temperature between the extremes without contracting.

Instead of being a paradox, therefore, it is the natural consequence of the workings of temperature upon the muscular current, that the

contractions should follow the order which they are found to follow, and being so, it is impossible to say that muscle is stimulated to contract either by heat or by cold.

6. *Muscular Contraction in relation to Light*.—Muscular contraction appears to be favoured by darkness, and not by light. It is in the darkness certainly, and not in the light, that contraction takes place in the irritable cushions of the sensitive plant; and it appears to be the same with the iris. *It appears to be the same*, for it is more easy to suppose that the iris expands under the stimulus of light, and so closes the pupil, than that this curtain is drawn and the pupil closed by sphincter fibres which have no existence. This explanation is supported by the authority of Bichât; it equally accounts for the phenomena; and it harmonizes with the known influence of light upon the sensitive plant.

7. *Muscular Contraction in relation to Chemical and Analogous Agencies*.—The evidence which belongs to this part of the subject is not so complete as could be desired, but, so far as it goes, it is quite in accordance with the premises. It is furnished by M. Eckardt (op. cit. p. 82). On analysing it, the simple fact is found to be, that the power of inducing contraction which belongs to any of these agents, is directly related to the power which the agent has of destroying the "irritability" of the nerve. The agents themselves act very differently—some by abstracting water from the nerve, some by altering the normal albuminous constituents of the nerve, and some in a more recondite manner: but all destroy the "irritability" of the nerve for the time being, and they do not induce contraction without destroying the "irritability." On experimenting with an acid, for example, the readiness with which contraction may be induced in the muscle by "irritating" the nerve with the point of a needle is found to diminish in direct proportion to the concentration of the acid, and when this concentration is sufficiently great to destroy the "irritability" of the nerve, then, and not till then, is the muscle made to contract by the *acid*. The experiment, in fact, is the precise counterpart of the one related previously, the only difference being that the agency of an acid is substituted for that of heat.

—So far, then, it appears to be altogether improbable that muscle is stimulated to contract by any of the several agencies which have been passed under review: but there are other and more difficult questions which remain in the back-ground, and these must be examined before any definite conclusion can be arrived at.

II.—In this part of the inquiry, the object is to examine into the real nature of muscular contraction, and to point out reasons for supposing that this contraction is nothing more than a passive physical consequence of the common molecular attraction of the muscle.

There are, undoubtedly, many facts which appear to stamp upon muscular contraction the peculiar impress of vitality. How else can the will have any concern in it? Why do the muscles lose so much of their contractile power after death if this power is not a vital endowment? Upon what mere physical hypothesis can the remarkable changes which are exhibited in the *form* of the muscular fibre be accounted for? And if muscular contraction is a vital phenomena,

then it is dependent upon stimulus, for life is the stimulus of stimuli.

As to the *will*, it is by no means certain that the action upon muscle is that of a stimulus. Undoubtedly *action* is involved in voluntary muscular contraction, but it is a question whether the *act* be *in the mind*, or *in the muscle*. The will *may act* by withdrawing something from the muscle, as well as by communicating something to the muscle. The will *may act* by suspending the muscular current for the time, and this supposition is in accordance with the premises. At any rate these premises are quite opposed to the idea that the will communicates anything to the muscle during contraction.

There is, undoubtedly, a diminished degree of shortening, and a loss of power after death; but it by no means follows from these facts that the contraction is dependent upon the stimulus of life.

The diminished degree of shortening after death may be nothing more than the simple physical consequence of the circumstances in which the muscle is placed. When a muscle contracts during life, the antagonist muscle either relaxes or opposes no resistance to this contraction. The blood also is fluid, and the intermuscular vessels are readily emptied when pressed upon by the contracting fibres. But after death the spasm is universal, and excess of contraction in any set of muscles is not favoured by the relaxation of antagonist muscles. After death, also, the full degree of muscular contraction may be prevented by the coagulated contents of some of the vessels. It must not be forgotten, however, that muscle can contract to a very great extent after death: thus the ventricular cavities of the heart are frequently obliterated by the contraction of the ventricular walls.

Nor is the loss of muscular strength after death a necessary proof that the contractile power of muscle is a vital endowment. Some loss of strength, indeed, may, or rather must, be the natural physical consequence of the circumstances in which the muscular fibre is then placed. In the first place, the fibre may be acted upon by the solvent juices which are present in muscle, and which Liebig has shown to be analogous in their properties to gastric-juice,—these juices acting upon the fibres just as the gastric-juice is sometimes found to act upon the coats of the stomach. Acted upon in this manner, the fibre may be partially dissolved, and to that extent weakened. In the second place, the dead muscle is yielded up to the process of decomposition, and the affinities of the *muscular* molecules may be weakened by the incipient or advanced resolution of these molecules into their constituent elements. Both these causes may combine to produce the result, and, combining, it is evident that the dead muscular fibre must suffer some loss of strength, not because the contractile power of muscle is a vital endowment; but because this power requires for its full manifestation a physical integrity of the muscular fibre which no longer exists.

It is obvious, therefore, that the muscles may lose much of their contractile power after death, without this power being of necessity a vital endowment.

When the movements of the living muscular fibre are considered,



the impression undoubtedly is that these movements are altogether mysterious and peculiar. Why the fibre in contracting should undergo little or no change in bulk, but gain in breadth what it loses in length; and why it should undergo such a remarkable degree of elongation in passing out of the contracted state, appears to be altogether beyond the scope of any physical explanation. The phenomena seem to be too wonderful to be accounted for by anything short of *life*—that mysterious something which, by being more mysterious, is made to account for all mysteries.

On reflecting upon these movements, however, a good deal of their mystery is dispelled, and, in the end, they are found to be capable of receiving a definite physical expression. Muscle, indeed, is made up of fibrin, and this fibrin, for all practical purposes, is identical with the fibrin of the blood. Now, this fibrin of the blood exists in a fluid form, and in a solid form. The fluid form is the living form; the solid, or coagulated form, is that which is assumed on death. Now, the question is, whether or not the fibrin of the muscle undergoes changes which correspond to these. One thing is certain, and this is, that *rigor mortis* is concurrent with the coagulation of the fibrin of the blood. More than this, there is good reason to believe that these two phenomena—*rigor mortis* and *coagulatio mortis*—are not only concurrent, but analogous. What, then, is the condition of the fibrin of the muscle *before rigor mortis*? Is it—like that of the fibrin of the blood—one of fluidity? What is the condition of the fibrin of the muscle in ordinary contraction? Is it, as in *rigor mortis*, one of *coagulation*? These questions naturally arise out of the history of the fibrin of the blood; and if they are answered affirmatively, then there is no difficulty in accounting for the peculiar changes of the muscular fibre.

If, then, the fibrin of the muscle be in a *solid* state during contraction, and in a *fluid* state at other times, it is easy to understand how the fibre may undergo that remarkable change in length which it undergoes when the contraction passes off; for the fluid fibrin will *run* where its course is least impeded, and this is in the direction of the tubes containing it. Again: if the fibrin becomes *solid* in contracting, the form of the contracting fibre need be no cause of difficulty; for this form may be the *natural* form of the fibrin, just as a rhomb may be the natural form of one solid substance, and a cube of another. Nor need there be any change of volume; for many substances solidify without undergoing any such change. Whatever is the real cause of muscular contraction, therefore, there is nothing in the changes of the muscular fibre which *necessitates* the conclusion that these changes are of a vital and mysterious character.

What, then, is muscular contraction? If it is not a vital phenomenon, is it a physical phenomenon? Directly or indirectly everything up to this point has tended to show that it *may* be a physical phenomenon; and there is, indeed, only one serious objection to this conclusion. This arises out of the law of the contraction. If the contraction is the consequence of any known physical attractive force, it is contended, the force of the contraction ought to *increase* after a definite law as the fibre contracts; but the very reverse is the

actual fact, and the force diminishes as the fibre contracts. Now, there is no doubt that the force diminishes as the fibre contracts; but there is every reason to doubt the correctness of the conclusion which has been drawn from this fact. The experiment of M. Schwann, which is usually cited as the proof, does not warrant any such conclusion. On measuring the force of contraction in the muscles of a frog's leg at different degrees of contraction, M. Schwann found that the force decreases as the muscles contract, and because it does this he concludes that the power cannot be that of molecular attraction. But he curiously forgets that the non-contracting, or imperfectly-contracting cellular substance of the muscle, and the inelastic fluids contained in the muscle, may oppose such a *resistance* to the contraction of the proper muscular fibres, as to mask completely the pure law of that contraction; and doing this his conclusion is altogether invalid. This experiment may indeed show *the degree of resistance* which is opposed to muscular contraction; but it is altogether worthless if it be supposed to show that the law of muscular contraction is essentially different from the law of known physical attractive forces; and it is upon this experiment alone that the idea of this essential difference in the law of muscular contraction is based.

But if muscular contraction is not a vital phenomenon, what is it? Is it the result of an *active* attractive force connected with the state of polar action? There are such attractive forces, unquestionably; but whenever they are present the polar action is also present, and whenever they are increased or diminished the polar action is also increased or diminished. It follows, therefore, that the contractile force of muscle cannot be of this kind, for the current of the muscle fails when this force comes into play, and when the force is manifested permanently, as in *rigor mortis*, the current is for ever extinguished. It follows, also, from the same evidence, that muscular contraction cannot be the result of any *active* physical attractive power, for there are no other forces of this kind besides those which are connected with polar action.

Only one course remains open, therefore, and this is to refer muscular contraction to that *passive* power of attraction which belongs to muscle in common with all matter; for this is the only power which is left after all active powers of attraction are done away with. This is the force which *must* come into play when the muscle ceases to be *resolved* by polar action; and this force is sufficient to account for all the phenomena which yet remain unaccounted for. It accounts for the *power* of muscular contraction, for it is this force, which, acting in the cooling bar of metal, is sufficient to draw in the walls of a bulging building. It accounts, also, for the phenomenon of *rigor mortis*—that phenomenon which is utterly inexplicable on the supposition that muscular contraction is caused by any kind of stimulation; for if this rigor is dependent upon simple molecular attraction, it is quite intelligible that it should come on sooner in cases in which the vitality of the system has been exhausted before death by old age, or by chronic disorder, such as consumption, than in persons who have been cut down suddenly in the full vigour of life, and that the fibre should remain contracted until it breaks up in the ruin of final decay,—for all that is



necessary for the continuance of this contraction is the physical integrity of the fibre. It accounts, that is to say, for those unexplained and seemingly contradictory facts which constitute the distinctive features of *rigor mortis*, and accounting for them, this very circumstance becomes a strong argument that molecular attraction is indeed the cause of muscular contraction.

—The conclusion, then, to which the whole of the previous argument tends is, that muscle is not *stimulated* to contract by any agency, physical or vital, but that contraction is a *passive* phenomenon which *happens* when muscle is left to the play of simple molecular attraction. In other words, *resolution* and not contraction would appear to be the *characteristic* state of living muscle,—this resolution being the natural consequence of the muscular current,—and contraction to be nothing more than the return of the muscle to the condition of those tissues which are never relaxed by currents.

III.—In this part it is proposed to examine the special muscular movements which are manifested in the coats of vessels, and to show, not only that these movements can be explained by no other law than that which has just been stated, but that the law gives the clue to the interpretation of “capillary motion,” and of the rhythm of the heart.

1. *Of the movements manifested in the coats of ordinary vessels, and of “capillary motion.”*—The manner in which the coats of vessels are affected by the several stimuli which act upon them need be no matter of obscurity. When the nervous energy is exuberant, as in joyous excitement, the skin is flushed; when this energy is depressed, as during fear, the skin is blanched. When the blood is rich and stimulating, as in plethora, the vessels are red and full; when it is poor and watery, as in anæmia, they are shrunk and empty. When the hand is held to the fire it flushes; when exposed to cold it becomes blanched. These phenomena appear to be utterly inconsistent with the idea that the muscular coats of the ordinary vessel are stimulated to contract by nervous influence, by blood or by heat; and there are many phenomena of the kind which are not less inconsistent.

On the contrary, this evidence appears to show that the coats of vessels expand under the influence of these several stimuli, and the test of the correctness of this conclusion is, that this view affords a clue to the interpretation of these mysterious movements of the blood which are independent of the impulse of the heart. In obtaining this clue, it must be assumed, not only that the vessels expand in this way, but that they expand to a far greater extent than the blood which is contained within them, and which is acted upon by the same causes of expansion; and, in order to this assumption, it must be remembered that the dartos and the subcutaneous cellular tissue generally, which are the analogues, or direct representatives of the tissues of which the coats of vessels are mainly built up, are relaxed (expanded) to a very remarkable degree under very small increments of heat, or any other stimuli—a degree to which there is nothing comparable in the blood or in any fluid under any circumstances. Let this be assumed then and the rest is obvious. When stimulated the vessel



expands to a greater degree than the blood contained within it, and the result is that certain vacua would be formed between the vessel and the blood, if more blood did move in to occupy the increased space. Hence, blood must rush into the *stimulated* vessel, and this equally, whether the vessels be acted upon by external heat, as by holding the hand to the fire, or by the natural stimulation of the blood itself within the vessels. In this way the *action* of the blood is to make a way for itself through the vessels.

2. *Of the Rhythm of the Heart.*—This problem is altogether inexplicable on the supposition that the ventricular systole is the result of stimulation, but upon the opposite theory it is easily disposed of.

The fact that the heart remains distended with blood during a full half of the time occupied in the rhythm is a strong argument that the blood does not excite the ventricular systole; and the history of plethora and anæmia are to the same effect. In plethora the pulse is full and slow; in anæmia empty and quick. In the one case, that is to say, the heart fills to distension with rich blood, and the pulse is deferred; in the other case, the heart takes in a small quantity of poor unstimulating blood, and expels it immediately. The facts are the very opposite of what they would be if the blood excited contraction, for then the pulse would be small and quick in plethora, and full and slow in anæmia. But if the blood provokes the ventricle to expansion by its stimulating properties, then it is intelligible that the heart should dilate more, and the dilatation continue longer when the blood is rich and warm, as in plethora, than when it is poor and watery, as in anæmia.

It may also be presumed that the ventricular systole is not excited by "nervous influence," if any argument may be drawn from what takes place when the nervous energy is more or less depressed, as during fear. Under these circumstances the heart beats hastily, and yet little blood is propelled into the vessels. The beats are perhaps doubled, and yet the skin is cold and pale. Now, under ordinary circumstances, the double number of beats would propel a doubled quantity of blood into the vessels, and the skin would be hot and red, instead of cold and pale; and hence the presumption that, in the apparently anomalous condition of the rapid pulse and pale skin which attend upon fear, the chambers of the heart are diminished in size by the contraction of the walls, and that they thus receive and propel less blood than usual. In other words, the ventricles appear to have contracted *without* nervous influence.

On realizing the actual phenomena of the heart's action, it appears still more improbable that the ventricular systole is caused by stimulation of any kind, and of the blood particularly. At the systole the arterial blood rushes through the coronary arteries into the coats of the heart, and the diastole occurs. The blood remains until it may be supposed to have lost its arterial properties, and then the systole returns. This is the simple fact. It is the *diastole* and not the systole which *appears* to be stimulated by the blood; and this view has the recommendation of affording the key to the rhythm of the heart.

Let it be supposed that the *ventricular* diastole is due partly to the

force with which the blood is propelled into the coronary arteries by the systole, and partly to the stimulation of the arterial blood within the vessels, and (to some extent) within the chambers of the heart. Let it be supposed that this diastole continues as long as the blood retains its arterial properties, and that the systole returns when these properties are exchanged for those of venous blood, and when the stimulus of oxygen is no longer present to avert the systole, and the rhythm is intelligible. Again, the systole restores the diastole; and again, in the same order, systole gives rise to diastole, and diastole to systole, as long as the ventricle can respond to the stimulus of the blood.

It even follows that the *auricular systole* must be contemporaneous with the *ventricular diastole*, for there is good reason to believe that this systole is more the effect of the *falling in* of the auricular walls upon the sudden withdrawal of blood from the auricles by the ventricular diastole, than of any special contraction in the auricles themselves. There is reason to believe this, partly from the absence of valves at the mouths of the veins opening into the auricles, and partly from the structure of the coats of the auricles. If the auricles had to contract *primarily*, it may fairly be assumed that there would have been valves to prevent the reflux of blood into the veins; if they had to contract *rapidly*, it may be assumed with equal propriety that the muscular structure would have been like that of the ventricle or any other muscle which has to contract rapidly, and not, as it is, like that of intestinal or other muscle which is only capable of contracting sluggishly. In this way there is no difficulty in accounting for the movements of the auricles; for the diastole of these organs (which is virtually contemporaneous with ventricular diastole) is partly due to the same cause—the rush of blood into the coronary arteries—and partly to the onward current of blood which sets in from the veins; and their systole is *mainly* due to the collapse of their walls on the passage of blood into the ventricle, at the ventricular diastole.

Hence, the rhythm of the heart receives a physical explanation, if the blood be supposed to counteract, and not to stimulate, contraction.

The same explanation applies even to the movements of the heart, or of a fragment of the heart, after removal from the body. Under these circumstances the air takes the place of the arterial blood, and the only difference is that the cardiac fibres are now stimulated to expand by the oxygen of the air instead of by the oxygen of the blood. If the heart be entire the circumstances are but little changed. The oxygenated air is driven into the coats of the heart through the coronary arteries (partly, at least) by the ventricular systole, and there it causes the diastole; but when this oxygen is replaced by carbonic acid, and the air acquires the negative properties of venous blood, then the diastole must cease, and the systole return. And thus diastole will follow systole, and systole diastole, for some time. Nor is the case very widely different when it is a mere fragment of a heart, which beats rhythmically. Acted upon by the atmosphere, the oxygen excites the electrical condition of the fibre, and induces expansion. Fresh supplies of oxygen, however, are required for the continuance of this action; and hence, it follows that the action will

fail, and be followed by contraction, when the oxygen in contact with the fibre is converted into carbonic acid. This contraction will displace the old and used-up air, and fresh air will come in to take its place. This fresh air will renew the action, and again place the fibre in the state of expansion, and this expansion will continue so long as the air retains its vivifying properties. Then the resulting contraction will replace the old air with new, and thus expansion will follow contraction, and contraction succeed to expansion time after time. In this way the air will act upon the interior as well as upon the exterior of the fragment, for the action upon the fibres composing the vessels, the cut ends of which are open to the atmosphere, will be to cause these vessels to expand, and to *draw*, as it were, the air into the interior—to draw it in and then expel it, much in the same way, and partly for the same reason, as the air-tubes draw in and expel the air which serves as breath.

There are other arguments of a similar significance to those which have been cited ; but sufficient has been said to show that the muscular contraction which is manifested in the coats of the vessels and in the heart, cannot be regarded as the result of stimulation ; while, at the same time, the opposite theory is found to give a clue to the explanation of two of the greatest mysteries in physiology, namely, “capillary motion,” and the rhythm of the heart.

IV.—In this part—the fourth and last—it is proposed to glance at the pathology of muscular contraction, and show that this is in conformity with the physiological premises.

Now the pathology of the disorders in which muscular contraction is in excess—namely, tremors, convulsions, and spasms, in their multifarious forms—is far too extensive a subject to be considered here, and all that it is possible to do is to glance at the substance of the evidence furnished elsewhere (Epilepsy, and affections of the nervous system, which are marked by tremor, convulsion, and spasm. Churchill, 8vo., 1854). This evidence, then, has been elicited from an examination of epilepsy, of affections allied to epilepsy, of the question of periodicity, and of treatment ; and this arrangement of the subject had best be preserved here.

1. In epilepsy, then, the condition of the circulation is habitually one of depression. The plethora of the butcher is never met with, and any vascular fulness, if such exists, is mere venous congestion. This depression is aggravated before the fit, and during the fit the condition tends either to syncope or asphyxia. If inflammation, or true fever, chance to be developed, so surely are the convulsions of epilepsy banished for the time. These conclusions are warranted by all the facts of the case.

With this condition of the circulation an active condition of the nervous system is incompatible, and this is quite in accordance with the actual facts. Sense and intellect are completely obliterated during the fit, and at all times they are under a cloud, or if this cloud is occasionally dispelled, and the patient is influenced by any real excitement, he is, for the time, relieved from his fits. *Agitation* may precede the fit, but never true excitement.

The muscles themselves are generally wanting in real tone.



The several causes of the malady are all exhausting, not exciting, in their character.

In a word, there is every reason to believe that the muscles of the epileptic contract excessively (as might be expected from the premises), because they are less stimulated than they ought to be, and not for a contrary reason.

2. In affections allied to epilepsy, whether they be marked by tremor, convulsion, or spasm, the same conclusions are arrived at.

The condition of the circulation during the paroxysm invariably tends to syncope or asphyxia, and inflammation or true fever is utterly incompatible with any form of tremor, convulsion, or spasm. Thus, tremor precedes fever, as rigor, and succeeds it, as subsultus; but it never accompanies fever. Thus, convulsion takes the place of rigor or subsultus, but it never happens in the intermediate hot stage of fever. Thus, the spasm of whooping cough disappears if pneumonia or bronchitis are developed, and returns again when the inflammation is over. In every instance the muscular turmoil is coincident with the opposite of vascular activity—the state tending to syncope or asphyxia.

As in epilepsy, so here it may be argued, that this condition of the circulation necessitates a condition of inaction in the function of the nervous system, and this presumption is fully corroborated both by the symptoms during life and the appearances after death. If inflammation of the great nervous centres has been present, the history of the case fully shows that this has been either before or after the tremor, convulsion, or spasm. The patient may be *agitated*, but he is never excited, in the true sense of the word.

The muscles, also, are found, as a rule, to be wanting in tone; and the so-called *exciting* causes are always *depressing* in their character.

Everything, indeed, tends to support the previous conclusions, and to show that in affections allied to epilepsy, as in epilepsy itself, and in ordinary muscular contraction, the muscles contract independently of any increased stimulation. The physiology explains the pathology, and the pathology confirms the physiology.

3. The phenomena of periodicity also point to conclusions of the same kind. The plant exhibits plainer and more numerous evidences of periodicity than the animal, and it does this, it is argued, because it has less of that innate life which enables the higher animals to be partially independent of the vivifying influences which are derived from the outer world. If man exhibits more evidences of periodicity than he ought to do, it follows, therefore, that he is shorn of some of that innate life which is the badge of distinction between him and the plant; and hence the periodicity of epilepsy or any cognate disorder is merely one proof that the system in which these diseases are manifested is less stimulated—less vitalized than it ought to be.

4. If, then, these diseases are of this character, it follows, as a necessary consequence—what, indeed, may almost be said to be proved by experience,—that bleeding, purging, or any lowering measures are not calculated to do any good; and that the only hope of benefit must be placed upon measures which will not only strengthen, but *rouse* the system,—a conclusion which is fully warranted by the experience of the author.

The only conclusion, then, which can be drawn from the consideration of those special muscular movements which are manifested in the coats of vessels, and of the pathology of the subject, is the same as that already drawn; and the final conclusion must be, *that muscle does not contract in consequence of the communication of any stimuli to the muscle, but it contracts because the common molecular attraction of the muscular fibre is no longer counteracted by the action of these stimuli upon the muscle.*

*On the Rhythm of the Heart in the Fœtus.* By Dr. FLEETWOOD CHURCHILL. ('Dublin Quarterly Journal of Medicine,' May, 1855).

Dr. Churchill has been lately engaged in investigating the rhythm of the fœtal heart in the Rotundo Hospital at Dublin, and the results at which he has arrived up to the present time may be stated in the following propositions:

1. That the pulsations of the fœtal heart range from 110 to 160 per minute, the average being somewhere about 136, and the audible sounds double, therefore ranging from 220 to 320.

2. That of the two sounds, the first is the weaker and less distinct; the second loud and distinct; the first audible only within a short distance of the fœtal heart; the second over a considerable extent of the uterine tumour.

3. That the rhythm may be expressed by dividing the entire period of a pulsation into four parts, and placing a dot under the figures, according to the succession of the two sounds, as 1 2̣ 3 4, and an accent over the louder sound.

4. That immediately after birth, the first and second sounds of the heart become equally loud and distinct from an increase in the first sound.

5. That the rhythm changes, and may be expressed thus: 1 2 3 4.

6. That this peculiarity of the rhythm continues for about a year and a half, and then gradually changes to that of the adult, expressed thus: 1 2 3 4, with the first sound stronger and louder than the second.

*On the Composition and Action of the Gastric Juice.* By MM. de GRUNEWALD and de SCHROEDER. ('Archiv. Générales de Medicine,' February, 1855; and 'Dublin Quarterly Journal of Medicine,' May, 1855.)

Notwithstanding the numerous investigations to which the gastric juice has already been subjected, the observations made by Drs. Grunewald and Schroeder on a woman affected with fistula of the stomach will be read with interest. This woman, aged 35, and enjoying good general health, weighed 53 kilogrammes (nearly 117 lbs. avoirdupois), and was suckling an infant at the time she was under observation. The fistula, which was of two or three years' standing, had doubtless been produced by a perforating ulcer of the

stomach. The quantity of gastric juice secreted was estimated, exclusively of the saliva,—65 grammes (a little more than two ounces) per hour,—at 584 grammes in the hour, or 14·016 kilogrammes (nearly 31 pounds) daily. This enormous proportion is much greater than that given by Bidder and Schmidt—6·4 kilogrammes (a little more than 14 lbs.) each day. The smallest quantity was collected in the morning, fasting; however, it was never less than from 40 (?) to 400 grammes (about 13 ounces) in the hour. The fluid then obtained was in general clear, serous, and colourless; at other times it was more viscid, it sometimes contained bile, without any sign of functional derangement being present. *Sarcinæ* were, with the aid of the microscope, pretty often observed.

As to the chemical constitution of the juice, which was investigated by Dr. Schmidt, the fluid obtained early in the morning, while the woman was fasting, was either neutral or slightly alkaline; after food was taken it was always acid. No hydrochloric acid was found in the analysis of several portions of gastric juice collected at different periods of the day; the presence of butyric and lactic acids is more probable. The following is a resumé of these analyses. In 1000 parts he found, water, 956·595; solids, 43·405;—the latter consisted of organic matter, 36·603; inorganic, 6·802;—the inorganic contained chloride of sodium, 4·633; phosphate of lime, 0·961; of magnesia, 0·260; phosphate of iron, 0·006; potash belonging to the organic substances, 0·363.

The organic substances consisted of coagulable albuminous matter (pepsin), sugar, butyric acid, uncoagulable protein substances, and lactic acid. The organic acids are not considered as primarily present in the gastric secretion, even as products of the ingested aliments; they vary in quantity according to the quality of the nourishment. Hydrochloric acid, on the contrary, has been regarded as an essential compound, although the analyses do not exhibit it, doubtless because it is easily neutralized by the alkalies of the saliva.

In one analysis Schmidt found free hydrochloric acid, but only in the proportion of two parts in 1000, a ratio ten times less than in the dog. As to the question whether the gastric juice prevents the saliva converting starch into sugar, as Bidder and Schmidt assert, the authors found that the action of the saliva was not destroyed; they however confirm the observation of those chemists that sugar is not to be found in the stomach of the dog, even after the ingestion of boiled starch.

The digestion of protein aliments was examined by introducing into the stomach through the fistula a certain weight of coagulated albumen, meat, &c., enclosed in thin linen bags; the loss of substance of the particles during a given space of time was thus observed as well as the changes which took place in the microscopic structure of the elements. It was thus found that for protein substances the solvent power of the human gastric juice is far inferior to that of the dog. Solution is perfected in the stomach of the dog in from two to four hours, while in that of man it requires nineteen or twenty hours. Raw meat is better digested by the human stomach than dressed meat, and veal than beef.



As to the microscopic alterations, the primary fasciculi were found, after an hour and a half, easily separable from one another, without having themselves undergone any change, the sarcolemma was destroyed. After two hours and three quarters, the primary bundles began to show transverse fissures; after three hours, only striated lamellæ were seen transversely; after three hours and a half, besides quadrilateral lamellæ, there were some primary fasciculi, longitudinally and transversely fissured, frequently denticulated at their extremities; after three hours and three quarters the primary fasciculi were two or three times longitudinally divided. After four hours and a half, there was scarcely any solid residue in the stomach, with the exception of a small number of primary bundles, much fissured both longitudinally and transversely, but still exhibiting the transverse striæ. At the end of three hours and a half, or four hours, the stomach was in general empty; the protein substances were then submitted to the influence of the intestinal secretion. As to the digestion of fat, the membrane of the cells is dissolved in the stomach, but the fat itself does not undergo any alteration. Milk, after three quarters of an hour, formed a thick coagulum, enclosing a large quantity of milk globules and of free fat. At the end of two and a half hours, the casein was observed in part as an amorphous substance, in part as membranous and transparent fragments, with some unaltered milk globules; at the end of three hours and three quarters, scarcely any remained in the stomach.

These changes, which in the stomach of this woman required three hours and three quarters, or four hours and a half, were completed in the stomach of a dog in two hours.

*An experimental inquiry into the nature of the Metamorphosis of Saccharine Matter, as a normal process in the Animal Economy.* By Dr. PAVY. (Proceedings of the Royal Society, 3d May, 1855.)

The saccharine matter met with in the animal economy is derived from two sources—from the vegetable kingdom, and from the liver of the animal itself; in each case being poured into the general circulation through the hepatic veins. The liver not only enjoys the power of forming sugar, but it likewise exerts (as shown by the experiments of Bernard) some modifying influence over that which is traversing its capillaries, and which has been absorbed from the food, by which it is transformed from *vegetable* into *animal* sugar, and thus rendered more apt for serving in the processes of animal life.

The sugar poured into the general circulation through the hepatic veins is conveyed to the capillaries of the lungs, where it in great part disappears, but never entirely so, according to very numerous analyses which the author has made on this subject. If the blood be traced onwards from the arteries through the systematic capillaries into the veins, the small amount of sugar which impregnates arterial blood will be found to be still undergoing a process of destruction; and what appears exceedingly interesting, this process of destruction is not carried on with equal activity in the different parts of the system

at large. In the capillaries of the chylo-poietic viscera, the destruction is so complete, that the blood in the portal vein may be entirely free from saccharine principle, when the blood returning from other parts, as that contained in the femoral or jugular veins, remains slightly impregnated. This curious fact has a bearing that will be presently adverted to, with reference to the views to be advanced concerning the nature of the metamorphosis of sugar in the animal economy.

The *principal* seat of destruction of saccharine matter in the animal system being located in the respiratory organs, seems at first sight to support the theory of Liebig—that sugar is one of those substances which undergoes a process of combustion, by its direct combination with oxygen, and its resolution into water and carbonic acid. Some experiments on the temporary obstruction of the respiration, and the examination of arterial blood before and after the operation, led the author to call in question this view, as he observed that, notwithstanding the supply of oxygen was cut off to such an extent as almost to occasion death, yet a considerable destruction of sugar took place in the lungs. This, coupled with the fact that a disappearance of sugar takes place in the systemic capillaries, and unequally so in different portions of them, induced him to push his investigations, and see if there might not be some other cause in operation in the living animal to effect the normal destruction of sugar, besides the direct chemical action of the oxygen absorbed in respiration. The results of these investigations, which were first directed towards the changes produced in blood normally containing sugar, injected through the capillaries of lungs removed from the animal, and artificially inflated with atmospheric air or oxygen gas, have induced the author to refer the metamorphosis of sugar in the animal economy to a process which is perfectly consistent and analogous with the well-known chemical bearings of this substance apart from the animal system.

In experiments which the author has now several times repeated, he injected blood removed from the right side of the heart of an animal—and therefore normally containing sugar—through the capillaries of the artificially inflated lungs of another, and found that, as long as the blood retains its fibrine, there is as much destruction of its sugar as would take place in the living animal; but that where the fibrine has been separated from the serum and corpuscles, the sugar ceases to be influenced by the presence of oxygen, or ceases to disappear during this process of artificial respiration. It would hence appear that something besides mere contact with oxygen is requisite for the destruction of sugar. But, in other experiments, he has found that oxygen is nevertheless a necessary agent concerned in the process of transformation observed during the arterialization of the blood that has not undergone spontaneous coagulation. It would therefore seem, in fact, that oxygen acts secondarily on the sugar through the medium of the fibrinous constituent of the blood; that it exerts some changes upon this azotized principle, which are capable of inducing the metamorphosis of sugar.

If we look to the ordinary chemical bearings of saccharine matter apart from the animal system, we find that an azotized substance

undergoing the molecular changes of decomposition, placed in contact with sugar, readily excites a process of fermentation, and converts it by a mere alteration of the grouping of its elements into another substance, one atom of sugar ( $\text{C}_{12} \text{H}_{12} \text{O}_{12}$ ) being resolved into two atoms of lactic acid ( $\text{C}_6 \text{H}_6 \text{O}_6$ ). We also find that sugar is not susceptible of oxidation, except under the influence of strong chemical reagents. Chemical analogy, therefore, would lead us to look upon the secondary reaction of oxygen as the more probable process of physiological destruction, especially when we take into consideration, that nowhere do we meet with such a constant series of molecular changes taking place as amongst the azotized constituents of a living animal. In the above-mentioned experiment of injecting fibrinated and defibrinated blood through an artificially inflated lung, when the blood is capable of undergoing the molecular changes of assimilation on contact with oxygen as in the living animal, the sugar in great part disappears; but so soon as the fibrine is separated by spontaneous coagulation, and the blood has thus lost its vital characteristics, oxygen is no longer capable of exerting any metamorphosing influence on its saccharine ingredients.

If the molecular changes occurring during the decomposition of an azotized substance be capable of converting sugar into lactic acid, why should not the molecular changes occurring during the building-up or elaboration of this same nitrogenized compound effect the same? Indeed, we have seen that the process of destruction is carried on to a certain extent in the systemic capillaries, and more especially in those of the chylo-poietic viscera, where the molecular changes of nutrition are also correspondingly carried on with greater activity than elsewhere. So that analogy and experiment would tend to show that the physiological destruction of sugar is owing to a process similar to fermentation induced by the molecular changes occurring in the nitrogenized constituents of the animal during life. And, in accordance with this, we find lactic acid present in the system, and largely separated from arterial blood by the muscular tissue, and the secerning follicles of the stomach.

As regards the lactic acid fermentation, it is well known that the presence of an alkali favours, whilst that of an acid retards the process. In two experiments on animals, the author injected carbonate of soda and phosphoric acid into the circulating current, and observed in the case of the latter that sugar immediately accumulated in the blood.

The preceding observations refer more especially to the changes that take place in the saccharine ingredient of the blood during life; and the author next proceeds to notice some interesting phenomena observable during the decomposition, and even the spontaneous coagulation of blood containing sugar.

If the blood of an animal normally impregnated with sugar be placed aside, and allowed to undergo spontaneous coagulation, on examining separately the serum and clot on the following day, it will be found, that although the serum may be largely saturated with sugar, the clot is entirely, or almost entirely, destitute of it. Now, as the clot is moist and remains to a certain extent infiltrated with



the serum from which it has partially separated, it would appear that even the molecular changes arising from the spontaneous coagulation of the blood are sufficient to effect the destruction of normal animal sugar. And this conclusion is strengthened by the fact, that in diabetic blood (the sugar of which, as would appear from other considerations also, is not so susceptible of metamorphosis as the healthy variety) the sugar does not disappear to a similar extent in the clot.

Under the changes of the decomposition of blood, normal animal glucose is very readily metamorphosed. The rapidity of the metamorphosis depends on the activity of the decomposition of the animal substances present, and when the destruction of the sugar is complete, the blood has assumed an *acid reaction*.

This acid reaction of decomposing blood is only observable in that which was previously pretty largely impregnated with sugar. It appears to be owing to the formation of lactic acid. Certainly it cannot be due to carbonic acid, for the reaction remains after exposure to a boiling temperature.

The disappearance of sugar in the manner just pointed out does not depend on the oxygen of the air, except in so far as this agent is concerned in exciting the decomposition of the azotized constituents of the blood; for the sugar disappears as rapidly when there is a small as when there is a large amount of surface exposed to the air. But if the air be carefully and completely excluded, no signs of decomposition of the animal parts of the blood are to be observed, and under these circumstances the sugar also remains. The disappearance of sugar is more rapid where the fibrine and corpuscles are present, than when the serum is exposed alone; and in accordance with this, the blood in the one case undergoes decomposition much sooner than in the other—a fact easily intelligible from the greater amount of azotized ingredients present.

If blood normally impregnated with saccharine matter be placed aside until signs of incipient decomposition are observed, and the sugar is beginning to disappear, exposure to a current of oxygen rapidly completes the total disappearance of the saccharine constituent. In this observation we have a further illustration of the analogy that appears to exist in the nature of the metamorphosis of sugar as a physiological process, and that which takes place chemically under the influence of an azotized compound, whose elementary particles are in a state of molecular transition. During life, the higher organic constituents of the blood are capable of undergoing the changes of assimilation on exposure to contact with oxygen, and there is a considerable destruction of sugar effected; for a short period after death these azotized constituents remain stationary and uninfluenced by oxygen, and with this, there is a corresponding suspension of the transformation of sugar; but, finally, the animal matter of the blood, on contact with oxygen, especially during a warm temperature, assumes a state of decomposition, the molecular changes of which again excite the destruction or metamorphosis of saccharine matter.

The sugar *disappears far less rapidly* from diabetic blood under the

influence of exposure to the atmosphere than from healthy right-ventricular blood. From these, and a few other observations which he has as yet been able to make on the blood in Diabetes Mellitus, the author, were he to hazard an opinion on the nature of that obscure disease, would be disposed to say that there appears to be a modification of sugar produced by the liver, which is not susceptible of undergoing the normal process of destruction in the animal system, and which, therefore, accumulating in the blood, is eliminated by the kidneys. The experiments of Bernard have shown that vegetable glucose (grape-sugar) is not susceptible of destruction in the processes of animal life, unless converted into animal glucose by the agency of the liver. Diabetic sugar would therefore seem to bear a resemblance in its physiological relations to vegetable, rather than to animal glucose.

*On the non-existence of Sugar in the Urine of the Fœtus.* By Dr. WILLIAM D. MOORE. ('Dublin Quarterly Journal of Medicine,' August, 1855.)

In some experiments, the particulars of which we subjoin, Dr. Moore was unable to detect the presence of sugar in foetal urine. He allows the insufficiency of the evidence, but the evidence, so far as it goes, is certainly at variance with the statements of Bernard and others. Arguing from these experiments, indeed, foetal urine would appear to be an albuminous fluid of feeble reaction, *free from sugar*, containing some of the usual salts of the urine, abounding in a highly nitrogenized principle, probably allantoin, but affording no urea, and depositing a most remarkably large amount of nucleated basement epithelium.

The urine for these experiments was procured in this manner. The urachus and urethra having been tied, the bladder was removed entire from the body, and having been carefully washed, was opened by a small incision made in the most dependant part, the contents as they escaped being received in a clean vessel.

"On the 30th of May, 1855," writes Dr. Moore, "I received from Dr. M'Clintock a foetal bladder containing about two drachms of clear urine; the reaction of the latter was slightly alkaline; when boiled the fluid deposited earthy phosphates, and was evidently albuminous, continuing opaque after the deposited phosphate was re-dissolved by the addition of dilute acid; the urine, when boiled with liquor potassæ, gave no evidence of containing sugar, nor did any reaction, indicative of the presence of the latter, take place when the urine was highly concentrated previously to the experiment. When freed from albumen, highly concentrated, and treated with nitric acid, it exhibited no sign of the presence of urea.

"On the following day I received a specimen of foetal urine, which I found to be very faintly acid; boiled and treated with dilute nitric acid, it yielded flakes of coagulated albumen; it exhibited no trace of either sugar or urea. The deposit was, on microscopic examination,

seen to contain a large quantity of epithelium; no blood corpuscles could be detected.

"I now determined to examine any specimen I might subsequently procure by means of the modification of the copper test proposed by Dr. Kletzinsky, the extreme delicacy of which I had ascertained by direct experiment. I therefore prepared some of the fluid recommended by him according to the form given by Dr. Dahl, in his 'Communications from the Chemico-Pathological Laboratory in Vienna,' which I had a short time before translated and published under the title of 'Heller's Pathological Chemistry of the Urine.' The test fluid is prepared by triturating together four parts of a saturated solution of sulphate of copper, six of glycerin, and eight of fused potash. Sulphate of potash crystallizes, and is separated by filtration; the filtered fluid is of a syrupy consistence, and of a beautiful deep blue colour. On boiling a few drops of it, with a solution of one part of honey, in one thousand parts of water, the copper is quickly reduced, and a similar reaction would evidently take place in a solution even still more dilute.

"I also satisfied myself of the delicacy and accuracy of this test in the following manner: Having boiled some drops of it for a few minutes with portion of urine known to be free from sugar, without any change having taken place, I stirred the mixture with a glass rod merely moistened with diabetic urine of the specific gravity 1.034, on which the characteristic changes were instantly produced.

"On the 18th of June, I received a small portion of foetal urine, having a very faintly acid reaction, and containing a minute proportion of albumen. Boiled with potassa fusa, it gave no indication of the presence of sugar, nor did any characteristic change take place on the application of Kletzinsky's test.

"The next specimen, which I received on the 16th of July, was tested solely by Kletzinsky's process. It was free from sugar, was slightly acid, and highly albuminous. Under the microscope it exhibited an enormous quantity of nucleated pavement epithelium. Some of the most beautiful specimens of this form of epithelium I have ever seen were deposited from foetal urine. Some blood and mucous corpuscles, and a few oil globules, were also visible. The quantity of albumen present, which on ebullition separated in large flakes, was much too great to be derived solely from the small amount of blood contained in the urine. The urine, when freed from albumen, highly concentrated and treated with nitric acid, afforded no evidence of the presence of urea; but when examined after the mode proposed by Dr. E. W. Davy, namely, by admixture with hypochlorite of soda, half a cubic inch of nitrogen gas was obtained from two scruples by measure of the urine; this is in the proportion of six cubic inches from the fluid ounce, which, if the nitrogen were derived solely from urea, would represent 3.873 grains of the latter principle. Had such an amount of urea, however, existed, I could not have failed in my attempt to form crystals of the nitrate from the specimen; and although Dr. Prout obtained a considerable quantity of uric acid crystals from a portion of foetal urine, and Dr. L. Lehmann observed



urates in that examined by him, the proportion of nitrogen above mentioned is far too great to have been derived from this source; it is, I think, much more likely that it proceeds from the decomposition of a peculiar nitrogenous principle, probably allantoin, present in the urine of the fœtus. Analogy would favour this view. Urea, which is more abundant in the urine of cattle than in that of man, has not as yet been discovered in the fluid of the allantois. 'The secretion of the non-respiring fœtus of the cow is,' observes Liebig, 'in a certain sense identical with the products secreted by the kidneys of the breathing animals. Urea represents carbonate of ammonia, from which the elements of two atoms of water have separated; allantoin represents oxalate of ammonia, from which the elements of three atoms of water have separated.' Should the correctness of the supposition I have advanced be confirmed by further experiment, it would, moreover, explain the discrepancy between the results obtained by Dr. Prout and Mr. Brande, for, as the composition of three atoms of allantoin is equivalent to that of one atom of uric acid, with one atom of urea and one of water, it is easy to conceive that crystals of uric acid might, under certain circumstances, spontaneously separate, while in another case the elements of the allantoin might retain their primitive arrangement.

"Allantoin is sparingly soluble in alcohol; I therefore think it probable that the appearances which led Dr. Prout to state that the alcoholic solution of fœtal urine 'gave, at first, faint and somewhat doubtful traces of urea,' which, 'on standing several days, became very distinct,' may have been due to the presence, not of urea, but of allantoin; indeed, immediately before, he states that alcohol 'was found to take up a principle strongly acid, and which readily assumed an imperfect crystalized form. I cannot venture,' he adds, 'to give this principle a name; it somewhat resembled the acid called amniotic, or rather allantoid, in some of its properties, but differed from it in others. Or it is possible that urea may have gradually separated from the alcoholic solution of allantoin, if such it was.'

"I have already stated, that the amount of nitrogen I obtained by Dr. Davy's very ingenious method would represent 3·873 grains of urea in the fluid ounce; now, as 79 parts of allantoin and 60 of urea contain an equal quantity (28 parts) of nitrogen, it follows that the six cubic inches of nitrogen would represent 5·099 grains of allantoin, as the amount of that principle contained in one fluid ounce of the fœtal urine under examination, for, 60 : 3·873 :: 79 : 5·099."

*On the transformation of the Cysticercus into the Tænia Solium.* By Dr. KÜCHENMEISTER. ('Weiner Med. Wochenschrift,' No. I, 1855.)

Dr. Küchenmeister has recently had an opportunity of proving experimentally upon man, that different varieties of cysticerci are converted into the tænia solium. The subject of the experiment was a condemned criminal. The cysticerci were obtained from the bodies of pigs, hares, &c., and given at intervals varying from five days to

twelve hours before the hour of execution. An examination was made forty-eight hours after death, when four small tæniæ were found in the duodenum, and six others, less perfectly developed, in the water with which the intestines had been washed. Seventy-five cysticerci had been administered, and therefore the rest had perished.

## V.

### REPORT ON MATERIA MEDICA AND THERAPEUTICS.

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*On the advantages of administering some medicines by placing them upon the tongue.* By Mr. WARDROP. ('Lancet,' May 12th, 1852?)

There are many circumstances which might be mentioned, in order to show the influence which some medicinal substances have on the animal economy when they are placed upon the surface of the tongue, these effects being caused by the absorption of the medicine, and its subsequent admixture with the mass of blood. Such phenomena are quite analogous to the effects produced by mercury or arsenic, whether these pass into the blood by the pulmonary, by the cutaneous, or by the absorbents of the alimentary canal. A gentleman, subject to what are usually called bilious headaches, had, during many years, seldom failed to obtain relief by taking sometimes two, and sometimes only one grain of calomel. He repeatedly found that there was a distinct difference in the length of time which the calomel took to relieve the headache, according as it was taken in the form of a powder put upon the tongue, or of a pill taken into the stomach. Another gentleman who had for many years suffered from dyspepsia, and who, for some years before Mr. Wardrop saw him, was in the habit of regulating his bowels by taking a pill composed of a couple of grains of aloes with myrrh, accidentally discovered that there was a remarkable difference in the effect of the pill when swallowed or when allowed to dissolve in the mouth. When taken into the stomach, it always created a good deal of pain in the whole course of the alimentary canal, and the evacuations were irregular both in number and in quantity; but when the pill was dissolved in the mouth, no other sensible effect was ever produced than one natural evacuation. Further experience convinced the author of the difference in the efficacy of medicines placed upon the tongue, or taken into the stomach, and led him to inquire into the cause, and endeavour to explain so important a phenomenon. The structure of the tongue pointed out that it possesses an abundant supply of absorbents. "The spirituous parts," observes the illustrious Haller, "more especially of vegetables, are received either into the papillæ themselves, or into the absorbing villi of the tongue; as appears from the speedy renovation of strength by liquors of this kind even when they are not taken into the stomach." This structure satisfactorily explains how medicinal bodies, when placed upon the



tongue, are absorbed and carried directly by the absorbent vessels of that organ into the venous circulation; whereas, when the same substances are taken into the stomach they are necessarily mixed with the food and juices contained in the alimentary canal, so that a more lengthened period must be required to separate them, and convey them by the absorbents into the thoracic duct, and thence into the venous system. Or they may pass unchanged, as has often been observed, out of the stomach, and in this unaltered state they are evacuated along with the excretions from the alimentary canal. This remarkable effect of medicines when placed upon the tongue, is strikingly illustrated in the administration of calomel; and it will be found that placing a very small quantity of it, say the sixth or even the twelfth part of a grain at short intervals, upon the tongue, such as every half-hour, the mineral is rapidly absorbed, and ptyalism more quickly produced than by any other mode of employing the calomel. These results of medicines are well known by the effects which croton-oil produces when applied to the tongue; and it is by no means improbable that the good effects of some medicines, when used in the form of lozenges, may be attributed to their absorption by the vessels of the tongue. All the circumstances regarding the difference and the effects of medicinal bodies, when conveyed to the venous system directly by the vessels of the tongue, or when they reach the blood by the more uncertain and circuitous course by the absorbents of the alimentary canal, appear to be worthy of being noticed, and may, it is not too much to hope, lead to some practical improvement in the mode of administering remedies. How far such differences will be found to result from exhibiting chloroform, the hydrocyanic acid, and the sulphates of quinine, iron, copper, and zinc, in the form of lozenges, and the advantages of using these medicines in such a manner, well merits further inquiry.

*On the internal use of Belladonna in poisoning by Opium.* By Dr. THOMAS ANDERSON. ('Edinburgh Medical and Surgical Journal,' and 'Indian Annals,' April, 1855.)

Dr. Anderson, it appears, was induced to make the important observations related in this paper by the statements of the late Dr. Graves respecting the beneficial action of belladonna in continued fever with contracted pupil and coma. Acting upon these statements, he gave belladonna to several patients suffering under these symptoms, and with very favorable results. This was at Edinburgh, in the winter of 1853. The idea then occurred to him that the same treatment might perhaps be beneficial in the coma with contracted pupils, caused by poisoning with opium; and he determined to test it by experiment as soon as a case of opium-poisoning should occur.

"This," he says, "I was soon enabled to do, as a patient, of whom I had charge, and labouring under delirium tremens, having received an over-dose of the solution of the muriate of morphia, became comatose. He had taken, in thirty-six hours, two ounces of the solution of the muriate of morphia, and it had been continued by the attendant after

sleep was procured. When I saw him he was in profound coma, his breathing was stertorous, amounting to no more than four or five per minute, and his pupils were contracted to mere points. His pulse was excessively weak, and rather slow; it was quite impossible to rouse him. I ordered him immediately the following mixture: Tincture of belladonna, six drachms, in five and a half ounces of water, of which an ounce was to be given every half hour. Three ounces of the mixture were administered with great caution, after which his pupils began to dilate. The six drachms of the tincture of belladonna were taken, and in four and a half hours after the first dose of it was given, the patient was in the following condition: The coma was entirely gone, respirations were between twenty-two and twenty-five per minute, the pupils were much dilated, the pulse had risen to nearly one hundred and twenty in the minute, and was also increased in strength. His countenance, also, from being cold and pallid, had become much flushed, and the whole body was much warmer.

"He replied readily and coherently to all my questions.

"He continued to improve for three days after, when rising suddenly to stool, he fainted, and before the assistance of the nurse could be procured, he was dead.

"A fortnight afterwards, I had another opportunity of testing my views. A woman, about fifty years of age, took, at 4 o'clock p.m., two drachms of laudanum, and at half past 5 p.m., three drachms more. She was brought to the infirmary at 8 o'clock p.m. After making vain attempts to rouse her from the coma, by walking her about, &c., the stomach-pump was used at a quarter past 8 o'clock. By this means her stomach was thoroughly evacuated, but no trace of opium was detected by smell or sight. It had probably been all absorbed. A current of electricity was then applied to her hands for nearly ten minutes, but without rousing her. I saw her at a quarter to 9 p.m. for the first time, and on being told that she had been poisoned by laudanum, I determined to try the effects of belladonna.

"At that time her pupils were contracted to mere points, her respiration was stertorous, ten per minute, the pulse was feeble, and the extremities rather cold. Between 9 and half past nine, I gave her one ounce of tincture of belladonna in three ounces of water, which was all swallowed, but with difficulty. In the course of the next half hour, two drachms more were administered. At 11 p.m., the first alteration in the size of the pupil was observed; the respirations had also then increased to twelve or thirteen in the minute, and the pulse was much stronger. The symptoms continued to improve till 2 a.m., when all indications of opium poisoning had disappeared. The woman was then sitting up in bed talking to the nurses, with pupils dilated to a little more than their natural size, and still slightly sensible to light. The extremities were quite warm, the pulse was about 100, and of good strength.

"She gave me a coherent account of her motives for taking the poison, of the amount of money she had spent in purchasing the laudanum, and the names of the druggists where it had been procured. She also replied sensibly to questions about her family, and the age and occupations of her children. She continued awake till nearly

4 o'clock a.m., after which she slept till 9 a.m. In the morning I found her pretty well, her pupils being no more dilated than they were four hours after the first administration of the belladonna. She complained, however, of nausea, but unaccompanied with vomiting. This symptom, along with the dilated pupils, had entirely disappeared in the course of two days. She was kept in the hospital, under observation, for ten days after the accident, at the end of which time she was dismissed, perfectly well. The tincture of belladonna used in both these cases was of the strength of four ounces of the leaves to two pints of rectified spirit, and prepared by percolation. Half a drachm is considered a full dose. I have seen dilatation of the pupil produced by a drachm given at once.

“So much, at present, for the action of belladonna on persons under the influence of opium. I will now very briefly notice some observations on the simultaneous administration of opium and belladonna, or its congener, hyoscyamus.

“My attention was accidentally directed to this subject some weeks ago, from a circumstance that happened to one of my patients. A man labouring under phthisis, and unable to rest at night from the violence of his cough, had the following mixture prescribed for him, as a soporific draught :

Sol. Mor. Mur., one drachm.

Tinct. Hyoscyami, two drachms.

Aqua Cassiæ, three ounces.

He took half of this draught at 11 p.m., but without obtaining any sleep, and before the morning the whole of it was given, but still with no effect. For two nights more, the same dose was repeated, but with no better result than at first. At last, I determined to try the effect of morphia alone; and accordingly I ordered a draught of thirty-five drops of the solution of the muriate of morphia, diluted with cassia water. After this he slept soundly, and therefore the same amount of morphia was continued for several nights with the same result—sound sleep.

“I mentioned this case to several of my friends, and two or three of them remembered similar cases which they had met with; but the sleeplessness following the simultaneous use of the medicines had been ascribed to some peculiarity in the constitution of the patient, and not to the opposite actions of the drugs upon the nervous system.

“In the ‘Association Medical Journal’ of the last week of November, 1853, I saw the following interesting case bearing on this subject; it is an abstract from the ‘American Journal of Medical Science:’—‘A child, nine years old, swallowed two suppositories, each containing two grains of opium and two grains of extract of belladonna. It went to sleep not long after. The mother awoke it at the end of four hours with great difficulty, when very free vomiting ensued, producing great exhaustion. The drugs were taken at noon, and at 7 p.m. the child seemed only a little fatigued and sleepy. It had eaten dinner immediately before swallowing the poison; and Dr. Coale (who relates the case) suggests that this may have retarded absorption.’



“Notwithstanding the very interesting, and, to me, almost conclusive nature of these experiments, I am not prepared, nor do I wish to say that I have discovered an antidote for poisoning by opium. In such cases, however, I believe that belladonna or hyoscyamus will be serviceable, on this supposition, that, if we give an agent whose action on the brain is opposite to that of opium, as soon as its minor physiological effects are developed, the evidences of the action of opium will disappear. It seems to me that these properties exist most markedly among the members of the natural family *Atropaceæ*. (See a paper on the *Solanaceæ*, by Mr. J. Miers, in the ‘Annals of Natural History’ for March, 1849. Also an abstract of a paper, by myself, on the *Solanaceæ*, in the ‘Annals’ for June, 1853, and the ‘Phytologist’ for May, 1853.)

“Many plants of this order, such as species of *atropa*, *hyoscyamus*, and *datura*, act as exaltants of the nervous system, increasing the rapidity of the respiration, and the strength and frequency of the pulse, causing delirium of various grades of violence, accompanied, till death, with dilated pupil, and terminating in coma, probably merely the result of exhaustion of the powers of the system. Now, these are effects quite the opposite of those observed daily as the consequences of the administration of opium. That drug exerts its poisonous influence as a depressent of the vital powers, diminishing the number of respirations, weakening the heart’s action, and causing coma, as one of the first alarming evidences of its effects.

“When talking lately to Dr. Garrod of my views of the treatment of poisoning by opium by belladonna, he told me, that in his opening lecture, delivered last October, in the University College, London, he had stated the converse of my idea,—viz., from the resemblance of the poisonous action of belladonna to delirium tremens, in which disease opium is a most approved remedy, it is probable that, in poisoning by belladonna, opium may be found advantageous.

“In conclusion, I will offer a few practical hints to any who may be inclined to test my views experimentally. That, taking the pupil as the index of the state of the brain, it is desirable to produce slight dilatation as speedily as possible.

“Now, in order to overcome the opposite effects of the opium, it is necessary to give doses three or four times greater than what would produce dilatation in a healthy adult. The first two cases that I have cited prove that no one need fear that their patients will thus be doubly poisoned. The tincture made from the leaves is the most uniform preparation. Four or six drachms of it might be given at first, and if that amount does not succeed in dilating the pupils in the course of an hour, the dose may be repeated. The careful application of a plaster of equal parts of the extract and lard to a blistered surface might be useful. Lastly, the internal use of small doses of atrophine—as, for example, one fourth or even one half of a grain in solution—will rapidly remove the contraction of the pupils.”

*On the use of Glycerine as an internal remedy.* By Dr. CRAWCOUR, of New Orleans. ('New Orleans Medical News,' and 'New York Journal of Medicine,' March 1855.)

"I wish to draw attention," writes Dr. Crawcour, "to the *special* action of glycerine on the economy, and the perfect safety with which it can be used as an internal remedy. For the past twelve months I have used it in every case of disease where formerly I should have used cod-liver oil, and with superior benefit; for while it seems to possess all the remedial virtues of this latter agent, it is its superior in taste, in not disordering the digestion, and in its property of combining with any other remedy.

"In several cases of phthisis, of scrofulous disease generally, in mesenteric disease in children, I have used it largely and successfully; and in children, its sweet and agreeable taste gives it a great advantage over cod-liver oil, the only agent I can compare it with in its therapeutic action. In addition to its special anti-strumous property, I find that it materially aids in the assimilation of salts of iron, especially of the iodide, and I now rarely order either iodine, or the iodide of iron, without combining them with glycerine. Quinine also is soluble in it, without the aid of sulphuric acid, and to some slight extent is divested of its bitterness.

"The dose in which I usually administer it is from one to three drachms three times daily, in an ounce of water; in from one to two drachms, it, in a short period, relieves the cough, improves the digestive powers, and appears to increase the fat-producing principle in phthisical patients; in larger doses it has in a few instances produced nausea; it is, however essentially necessary to its successful employment that it be obtained pure, and this is a matter of some difficulty, for it is ordinarily the result of the preparation of the common lead plaster, and consequently contains traces of lead."

*On the use of Cod-liver-oil Oleine.* By Dr. LEARED. ('Medical Times and Gazette,' July 21, 1855.)

Dr. Leared advocates the use of the oleine of cod-liver oil, instead of the simple oil, upon two grounds:—First, that the oleine is the medicinal part of the oil; and, secondly, that it is far less likely to disagree with the stomach of the patient. Dr. Leared holds peculiar views respecting the digestion and assimilation of oils and fat, and these views have led to the therapeutical employment of the oleine. He holds that the operation of the pancreatic fluid is to resolve the oils or fats into their constituent principles—stearine, margarine, oleine; that the oleine is alone available for nutritive purposes, and that the rest are simply excrementitious. Dr. Leared says, moreover, that his experience goes to support this theory; that the separated oleine is, to say the least, quite as efficacious as the oil; and that it was taken readily by persons who could not take the oil.

Cod-liver oil yields about 75 per cent. of oleine, and the oleine may

be obtained by submitting the oil to a low temperature, and separating it by pressure from the semi-solid mass.

*On the physiological action of Chloride of Ammonium.* By Dr. ALEXANDER LINDSAY. ('Glasgow Medical Journal,' Oct., 1855.)

Wishful to ascertain the influence of medicinal doses of chloride of ammonium upon the healthy organism, Dr. Lindsay and two of his pupils agreed to make an experiment upon themselves.

"Daily, for a week previous to the experiment, the state of the appetite, the nature and amount of the food, the condition of the bowels, the frequency of the pulse, with the amount and density of the urinary secretion, were carefully noted. The medicine was then taken for a week, and similar observations recorded. The amount taken was in one case 18 grains per day, a second  $13\frac{1}{2}$  grains, and the third 9 grains. These quantities were divided into three equal doses, and were swallowed dissolved in two ounces of water. No comparison of the results was made till the observations were concluded. The following is a brief summary of these, from the notes now before me:—

"On the second day after beginning the medicine, a buoyancy of the system was experienced that rendered ordinary pursuits a pleasure, and fitted body and mind for increased exertion. The uniformity of this result was the more remarkable as the experimenters represent types of the nervous, sanguineous, and lymphatic temperaments respectively. The feeling was least developed in the last. He employed the smallest dose. In all, the appetite was much improved. Where the smallest quantity of the salt was taken, the amount of food was doubled. The feculent discharges were in all much augmented. The mucous follicles of the intestinal tube seemed to be stimulated to a much increased secretion. In two, the force and frequency of the heart's action were diminished. The rate of the pulse in the gentleman employing the smallest dose was accelerated. In all, the chloride increased the urinary secretion. It cannot, however, be classed as a renal hydragogue. The increase of fluid ranged from six to twelve ounces in the twenty-four hours. In the two cases, where the largest and smallest doses were used, it acted as a renal depurant, the excess of solids varying from 70 to 160 grains daily. In the other no change in this respect was noticed; but it may be necessary to remark, that the effect on the bowels appeared to be greatest in the individual making use of the medium dose."

*On Albumen as a Cholagogue.* By Dr. R. GIESELER, of Göttingen. ('Dublin Quarterly Journal,' August, 1855.)

"Bernard's experiments, showing that this substance is assimilable only through the intervention of the hepatic function, immediately suggested to me the idea that in albumen we might find an adequate excitant of the liver. I inferred, first, that fatty nutriment, and in a higher degree albuminous articles of diet, must be avoided in in-



inflammation of the liver; and secondly, that in torpid conditions of that organ we might possess in albumen a remedy capable of stimulating it to increased activity. If to the liver be assigned the task of rendering albumen adapted to assimilation, this substance must be a stimulant of it, which will, *mutatis mutandis*, set its function to work, in the same manner as the administration of saline medicines does that of the kidneys. It is scarcely necessary to add, that the establishment of these results by experience must secure to albumen not merely the character of an adequate stimulant, but also pre-eminence over all other so-called cholagogues, since the action of the latter is very uncertain.

“I think it unnecessary to demonstrate the remarkable efficacy of albumen in this respect by the recital of cases, since it was, as I soon learned, already known to our predecessors. It, however, appears to me not unimportant to point out the source whence it would appear the recommendation to employ albumen as a remedy in jaundice was originally derived. Charles White, in his work on ‘The Treatment of Pregnant and Puerperal Women,’ states, that he once suffered for several weeks from jaundice, and was very much reduced. Soap, aloes, iron, and rhubarb, had been taken without the least benefit. A naval officer, happening to visit him, assured him he would cure him in a short time. He told him, in fact, that while on a voyage some time before, he was attacked with the same disease, and had in vain used the remedies prescribed by the surgeon of the vessel. A Spanish physician of the island of Minorca then advised him to take every morning, while fasting, two raw eggs, both yolk and white, in a glass of water, and to repeat the dose with one egg every four hours during the day. He followed this advice, and in three days his motions were again coloured with bile. White tried the plan suggested, and found the effect attributed to the albumen to be confirmed; in three days the fæces were coloured, which they had not been for six weeks before. He continued the use of the eggs for some months. He subsequently recommended the remedy to several patients, and always with good effect, except in cases in which the jaundice proceeded from the presence of gall-stones. So far for the testimony of Mr. White. In the more modern treatises on therapeutics I have not been able to find any allusion to this application of albumen; the present communication cannot, therefore, be considered superfluous. A few of the older works recommend, not white of egg, but the yolk, probably on account of its yellow colour. It is indeed possible that the action of the liver may be excited, not by the vitellin of the yolk, but merely by the albumen of the egg, with which Bernard experimented, and which White recommended in jaundice. Should this supposition prove correct, it would explain why the remedy lapsed into oblivion, and would furnish an important proof in our day for the often misunderstood truth, that practical results do not become the property of science or art, until they are referred to correct principles.”

*On Asparagus as a Diuretic.* By Dr. JEAFFRESON, of Leamington.  
(‘Assoc. Med. Journ.,’ 11th May, 1855.)

Dr. Jeaffreson states in this paper that for sixteen or seventeen years he has found tincture of asparagus to be a very valuable diuretic. The tincture is made in this way.

Take of dried tops of asparagus, five ounces; proof spirit, two pints. Take of fresh tops of asparagus five pounds. Bruise and press out the juice; evaporate at a low temperature till reduced to one pint, and strain. Lastly, add a pint of rectified spirit.

“The peculiar odour communicated by this substance to the urine, in a remarkably short time, is perhaps as familiar to the laity as the profession. It was this fact that first led me to think that asparagus might constitute a valuable adjunct to our list of diuretics; if not indeed by virtue of any specific diuretic quality it possessed, at least by its power of directing other agents of acknowledged diuretic power to the kidneys.

“On referring to such authorities as fell in my way, I found that, whilst some mentioned asparagus as a diuretic in general terms, without any specific reference to its medicinal administration, others omitted entirely to notice this plant, and some others denied its diuretic properties entirely. It appeared to me evident that any deductions drawn on this subject had been founded entirely on its effects as an article of food, and not upon any direct experiments of its medicinal administration. The fallacy and uselessness of such deductions is sufficiently apparent; upon generalizations so vague, we might have discarded numerous of our best remedies as deleterious, innocuous, or useless.

“Suffice it to say that, after some sixteen years’ experience, I have found the tincture of asparagus a useful adjunct to our diuretic remedies. In many cases I have found it possessing direct diuretic properties when taken alone in water; but, in still more instances, I have found it most useful in promoting the diuretic properties of other drugs, as I conceive, by directing them at once to the kidneys. I have repeatedly in my own practice, as also in consultation, simply added from half a drachm to two drachms of tincture of asparagus to each dose of an unsuccessful diuretic, and found that copious diuresis was the result.

“The exact loss by weight in drying the plant is eleven parts out of twelve; in other words, that twelve parts by weight of the fresh shoots are only equal to one part of dried. I have not tried the infusion of the dry shoots, but should think them worthy of trial.

“The tincture of asparagus presents the advantage of being capable of combination, so far as I know by experience, with every diuretic substance in use, be it from the animal, the vegetable, or the mineral kingdom.”

*On the Saoria and Tatzé as new Tænifuges.* BY MR. HEPP. ('Bull. Gén. de Thérap.' 15th and 30th of July, 1830; and 'Medico-Chirurgical Review,' July 1855.)

The *Saoria* (*sauarja*) is the ripe and dried fruit of the *maesa* (*bacobotrys*) *picta* (Hachstetter). According to M. Schimper, it is found throughout Abyssinia, at the height of 7000 to 9000 feet, never below 6000 feet. The fruit is an ovoid drupe, covered over two-thirds of its extent by the calyx, and of a greenish-yellow colour. The seeds are turbinate, angular, flattened at the apex, and covered by a resinous substance in ellipsoid grains. The long diameter of the fruit is from three to four millimetres, the short diameter a little less; it is then about the size of pepper. The taste is at first somewhat aromatic, oily, and astringent, and leaves for some time afterwards a tolerably persistent acid sensation in the pharynx. M. Schimper states that the dried fruit is administered in powder, in doses of 32 to 44 grammes; that it purges, and kills and expels the worm entire, without affecting the health of the patient.

With a view to determine its action upon Europeans, Mr. Hepp collected observations upon its use from different medical men, and in the paper under analysis thirteen such observations are recorded. In two of these, however, no worm was suspected to be present; and in three, although suspected, no portion of it had been seen. In the remaining eight cases the parasite was expelled; but, as is commonly the case after other tænifuges, the head of the worm was not found. In Abyssinia, according to M. Schimper, the saoria passes for a tænicide, and one of the observations recorded appears corroborative of the fact. The other effects produced in the cases related were the following:—nausea, five times; vomiting, once; colic, five times, violent in one case; three to five alvine evacuations; and in three cases, general feeling of illness, with peculiar sensations explicable otherwise than by reference to the saoria were observed; and in several cases, with the exception of the purging, the symptoms referred to were wanting. The medicine exercises a special action upon the urine, imparting to it a violet colour.

The following is given as the mode of administration:—A moderate regimen the previous day; in the morning, fasting, thirty grammes of the powder of saoria suspended in a liquid. Should nausea occur, it may be allayed by some mild aromatic. Ordinarily, in two or three hours liquid stools will occur, in which the tænia will be found dead. Should the bowels fail to act, castor oil may be administered in the course of the day. A mild regimen during the day; and on the morrow, if the stools have been scanty, some further evacuations may be obtained, with a view to drive out the remains of the worm not expelled the previous day. If the head of the worm is wanting, there is no objection to repeating the dose in four to eight days' time.

The following are the conclusions drawn:

1. The saoria is more sure than our indigenous tænifuges, though we cannot yet call its action constant. It would appear to be tænicide.



2. Its action is mild, seldom accompanied by disagreeable effects ; and it is not difficult to swallow.

3. It may be fearlessly and readily administered to young children and to females, as well as to persons with a shattered constitution and weak digestive canal.

4. These different properties bespeak its superiority over our indigenous tæniifuges.

5. It is preferable to kousso, on account of its milder and yet tænicide operation, and from the lower price at which it may probably be obtained, since it is much more extensively distributed than the kousso. Its more ready and longer preservation is equally an advantage over both this and the fern-root.

6. Time alone can pronounce whether its operation is radical or only palliative.

The *tatzé* is the fruit of the *Myrsina africana*, a native of Abyssinia, the Cape of Good Hope, the Azores, and Algeria. It is a more disagreeable remedy than the saoria ; and in six cases in which it was administered, the patients did not complain of any colicky symptoms being induced, and its purgative operation is not constant. It imparts an inky tinge to the urine. It is said to be tænicide. It succeeded in expelling the tænia in each of the six cases in which it was given, and in one of these, several other active vermifuges had failed. The medium dose of the powder of *tatzé* is fifteen grammes, followed, if necessary, by a dose of castor oil.

*On the substitution of Zinc for Lead in Diachylon Plaster.* By M. de MUSSY. ('Bull. Gén. de Thér.,' 1854; and 'Medico-Chirurgical Review,' July 1855.)

During a stay at the thermal springs of the Pyrénées, Dr. De Mussy was struck with the fact that where the baths were used by persons employing diachylon plaster, all those parts of the skin which had been in contact with it became covered with a thick layer of sulphuret of lead, which was very difficult of removal, and he was led to inquire how far it was prudent to maintain these saturnine compounds for a long time in contact with large absorbing ulcerated surfaces. He refers to an instance in which, on two different occasions, lead colic was induced by such an application. At the suggestion of this physician, M. Boileau endeavoured to form a plaster with a base of zinc instead of lead. A solution of white soap was brought in contact with a solution of sulphate of zinc, an abundant precipitate of oleo-margarate of zinc fell, which, being washed and dried, was combined with the various other substances which enter into the formation of diachylum ; augmenting, however, the proportion of oil and wax in order to preserve a proper consistence to the plaster.

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